Political Preferences in Adverse Conditions

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ABSTRACT

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Why do voters change their political behavior after negative events such as natural disasters and crime victimization? The extant literature tends to focus on how citizens punish or reward the incumbent based on a model of (mis)attribution of responsibilities. This approach overlooks the fact that affected voters might change their political preferences after the negative shock. Departing from the existing literature, I argue that affected citizens, in addition to evaluating incumbent performance, are also selecting the political leader they believe can most enhance their well-being after the negative event. In particular, I hold that affected voters focus on improving their living conditions, which leads them to pay attention to the policy issues that can help them achieve that goal. As a consequence, victims are more likely to prefer candidates better able to address these new policy preferences. Under adverse conditions, these individuals will vote for political candidates whom they would not select under other circumstances. In each of the three chapters of this dissertation, I provide evidence to support different aspects of this main argument.

In the first chapter, I study the political consequences of natural disasters. According to my theory, citizens affected by catastrophes seek to reduce the gap between their living conditions before and after the disaster. This leads them to focus on welfare and social policies – for example, the construction of new housing. Consequently, they are more inclined to vote for parties or persons associated with those measures, typically left-wing candidates. To test this argument, I use a natural experiment created by flash floods that occurred in Chile in 2015, which produced random variation in exposure to the natural disaster. I then measure voters' political preferences using a conjoint survey experiment, and find that disaster victims are more likely to prefer left-wing candidates. In addition, grounded in two months of fieldwork in the affected area, I provide qualitative evidence that illustrates how disaster victims emphasize the importance of welfare policies that can improve their standard of living.

In the second chapter, I show how disaster victims after the 2010 earthquake in Chile

select housing and not infrastructure as a top priority after the catastrophe. These results help us better understand why disaster victims are more likely to vote for left-wing politicians: affected citizens are particularly concerned about the reconstruction of their houses, and in consequence, should be more likely to vote for candidates who can be linked with those specific welfare policies. To study how the earthquake modified victims' political priorities, I rely on survey data before and after this negative event comparing exposed and unexposed counties.

In the third chapter, I study how crime victims change their policy preferences. I show that affected citizens are more likely to support strong-handed measures to reduce crime, such as allowing state repression. These results reveal that exposure to crime can change what people think the state should be allowed to do, which can have important political implications. To study the impact of crime on victims' preferences, I use panel data from Brazil and I implement strategies for reducing sensitivity to hidden biases, such as focusing on individuals who were not crime victims during a previous wave.

Contents

List of Figures

List of Tables

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Preface

Residents of the developing world are frequently exposed to adverse conditions generated by negative events, such as natural disasters and crime waves. These circumstances often reduce citizens' incomes and diminish their living conditions. This dissertation seeks to answer the following question: Do voters change their political preferences and behavior after negative events, and if so, how and why?

The extant literature focuses on how voters punish or reward the incumbent candidate or party after a negative shock based on a model of (mis)attribution of responsibilities. Such shocks, however, can also modify victims' political preferences, which can then affect their electoral choices. This dissertation, thus, helps us understand what kind of political leaders disaster and crime victims prefer. I argue that affected citizens, in addition to blaming or rewarding incumbents for their response to the negative shock, also select the candidate who they believe can best enhance their well-being. Specifically, I hold that victims focus on improving their standard of living, affecting the policy issues they most care about. In consequence, victims are more likely to prefer candidates who can better address their new policy preferences.

In the first chapter, I study the impact of natural disasters, of growing concern due to climate change, on their victims' political preferences. According to my theory, those affected by catastrophes seek to reduce the gap between their living conditions before and after the disaster. This leads them to focus on welfare and social policies – for example, the construction of new housing.

It is difficult, however, to study the consequences of disaster damage because they can be correlated with several unobserved characteristics. To analyze the impact of these negative events, I focus on flash floods that occurred in Chile in 2015, using the natural experiment created by the flood damage to identify exposed and unexposed citizens. I then

implement a conjoint survey experiment to measure the impact of the disaster on voters' political preferences.

My analysis shows that material damage due to the flood increased the probability of preferring left-wing candidates by 12 percentage points – candidates whom respondents could associate with the welfare and social policies that can improve victims' living conditions. Qualitative evidence from interviews further illustrates how disaster victims emphasize the importance of welfare policies that can improve their living conditions. To increase external validity, I compare these findings to the actual electoral response to the flood and provide survey evidence from a different natural disaster. My research demonstrates that in addition to punishing or rewarding incumbents, victims select the candidate who can provide the social programs they need. In consequence, left-wing parties and candidates should have a natural electoral advantage after disasters.

In the second chapter, I focus on the mechanisms to understand disaster victims' electoral choices. I provide evidence that affected citizens strongly emphasize housing as a top post-disaster priority, as a result of which they should be more likely to vote for politicians who can be associated with those specific welfare measures. In particular, using survey data before and after the 2010 earthquake that affected south-central Chile, I show how affected voters are more likely to prioritize housing but not infrastructure after the disaster. These are surprising results because of the devastating consequences of this catastrophe, which destroyed schools, hospitals, roads, airports, and bridges. These findings reveal that disaster victims have myopic and selfish interests since they only emphasize the reconstruction of their houses, and not the improvement of their communities.

To study how the earthquake modified victims' priorities, I use survey data before and after this event, comparing exposed and unexposed areas using a synthetic panel. This methodology addresses a potential issue that arises when using survey data: namely, that because of sampling variability, and the use of particular geographic areas as treated and control groups, there can be imbalances within each group across time, which can translate into bias when estimating a difference-in-differences. I rely on advances in optimal matching and mathematical programming to construct a synthetic panel using three waves of surveys, two conducted three and six months before the earthquake, and one implemented

three months after the disaster. The primary goal of the synthetic panel is to generate comparable groups of people before and after the earthquake, so sample composition is similar across periods.

In the third chapter, I examine the impact of crime victimization on victims' policy preferences, which is one of the dimensions of the main argument. I find that crime victims are less likely to support democratic values and, thus, more likely to support strong-handed crime-reduction policies. To address certain methodological concerns that arise when studying the effects of crime, such as reverse causality and serial victimization, I use panel data from Brazil to compare crime victims and unaffected respondents, and I focus on individuals who had not been crime victims during the previous wave. Additionally, I reduce sample heterogeneity to decrease sensitivity to hidden biases by comparing citizens from the same neighborhoods.

Also, I provide survey evidence from 18 Latin American countries to improve external validity. This chapter's findings may have important political implications: if crime victims are more likely to support a repressive state, a rise in crime during an election cycle might be exploited by right-wing candidates who propose iron-fist policies for combating crime.

These three chapters show that affected citizens are modifying their political preferences, which can impact their electoral choices. Because a negative event changes the policy issues victims most care about, affected voters will be more likely to prefer candidates who can better address their new policy preferences. For example, disaster victims are more likely to support the distribution of welfare and social policies (i.e., housing), and as a result to vote for left-wing politicians.

Chapter 1

After the Flood: Natural Disasters and Electoral Choices in Chile

Abstract

What candidate characteristics become more important to voters after a natural catastrophe? Even as climate change has increased concerns about the frequency and intensity of natural disasters, the effects of these catastrophes on voter behavior is not yet well understood. The extant literature focuses on how voters punish or reward incumbent performance based on a model of (mis)attribution of responsibilities. However, disaster victims might also pay attention to specific candidate characteristics when making electoral choices. To analyze this hypothesis, I use a natural experiment created by the floods that occurred in Chile in 2015 to take advantage of random variation in citizens' exposure to a disaster. I then capture voters' electoral choices using a conjoint survey experiment. The findings show that material damage caused by the flood increased the probability of voters selecting left-wing candidates, who can be associated with social policies that can ameliorate the repercussions of the catastrophe.

1.1 Introduction

The question of how negative circumstances change citizens' electoral choices is a central inquiry in any democratic country, especially in places frequently exposed to situations that can damage people's living conditions. Natural disasters are one of the most devastating of these negative events, generating significant costs for the countries and citizens affected. This is evident in regions like Latin America, where between 1970 and 1999 the annual cost of natural catastrophes ranged between $700 million and $3.3 billion (Charvériat, 2000). Additionally, according to NASA, climate change will increase the likelihood of natural disasters in the future,[1] which could lead to a greater risk of inland flooding and tropical cyclones (Van Aalst, 2006).

Disaster victims face a variety of negative effects on their living conditions, such as income reduction, the deterioration of public services, and post-traumatic stress disorder. Furthermore, natural catastrophes also have an impact on electoral outcomes (Achen and Bartels, 2016; Ashworth, Mesquita, and Friedenberg, 2014; Healy and Malhotra, 2010).

Despite these known effects, the mechanisms underlying voter behavior after natural disasters are not yet well understood. Why do citizens change their political behavior after natural disasters? Do catastrophes make candidates with certain characteristics more attractive to voters? The extant literature focuses on the evaluation of incumbent performance. In particular, it has mainly explored whether voters punish or reward the ruling candidate after a negative shock: for example, if the disaster increased or decreased support for the candidate or party in power, as measured through the incumbent vote share. Most of the findings showing a positive or negative effect on incumbents' vote share have been attributed to voters' (mis)evaluations of previous events.

This previous research has failed to consider how the characteristics of the candidates themselves may play a role in electoral decision-making. According to this hypothesis, disaster victims might also pay attention to particular candidates' attributes when making electoral decisions. Thus, affected citizens might not only sanction incumbents but also

[1] "The Impact of Climate Change on Natural Disasters", Earth Observatory, NASA, Retrieved May 30, 2017.

select candidates based on the new context. For example, are the age, profession, ideology, or experience of the candidates important to voters after a natural catastrophe? We do not have a clear answer to this question.

Trying to understand which candidate characteristics become more important for voters after a natural catastrophe presents multiple methodological challenges. First, even though the origin of natural disasters might be exogenous to incumbents' performances, natural disasters are not randomized experiments. Indeed, damage incurred by disaster victims can be correlated with a variety of characteristics: for example, poor individuals might be more likely to live in high-risk areas, such as close to a river or near the mountains. Therefore, certain voters might have a greater chance of being exposed to a natural disaster.

Second, previous research designs do not tend to account for the importance of sample homogeneity for drawing more credible inferences. Ideally, an observational study should compare subjects from the same natural blocks, such as students from the same school or patients from the same hospital (Pimentel et al., 2015). By drawing units from the same homogeneous sample, the treated and control groups may have similar distributions of unobserved covariates, which will improve comparability between units and reduce sensitivity to hidden biases (Keele, 2015; Rosenbaum, 2011). Few studies, however, take this point into account: rarely do treated and control units come from homogeneous samples, which increases, by design, the impact of unobservables.

The third methodological challenge is that the characteristics of candidates might be endogenous to the disaster. For example, it is possible that parties tend to nominate candidates with certain attributes in districts exposed to catastrophes. As a consequence, it is important to isolate candidates' characteristics from the disaster itself.

My research design addresses each of these concerns, focusing on a particular case of flooding in northern Chile. In March 2015, unseasonably heavy rains in that region of the country triggered flash floods, causing severe damage in numerous cities and towns. Copiapó, the capital city of the Atacama region, was severely affected by this natural disaster. I focus on a district called Paipote, the most affected area of the city of Copiapó. Some parts of Paipote, however, were not exposed to the flood because of haphazard circumstances. This provides an opportunity to compare voters indirectly affected by the flood

(those who experienced isolation and a scarcity of supplies for several days but no material damage) with those who were directly affected by the disaster (those who experienced material damage in addition to isolation and scarcity).

This case allows us to address two of the aforementioned methodological challenges. First, the as-if random nature of exposure to the flood allows us to better identify the political consequences of a natural disaster: unexposed people had not sorted or selected their houses based on their expectations of being affected by a disaster since the magnitude and trajectory of the flood were unpredictable. Second, because Paipote is a homogeneous low-middle income town, the comparability between voters and, therefore, our ability to draw credible inferences from the data, increases.

To better understand how candidate characteristics may become more relevant to voters after a natural disaster, I conducted an original survey with an embedded conjoint experiment in the more- and the less-affected areas of the town three months after the disaster. The main goal of the conjoint analysis was to determine how people value different candidate attributes when making electoral decisions. By randomizing candidates' characteristics, the conjoint experiment allows us to identify the effects of each of these attributes in a mayoral race (Hainmueller, Hopkins, and Yamamoto, 2014). Furthermore, by using hypothetical candidates who were not nominated by political parties but rather randomly generated, this approach helps address the third methodological concern.

I argue that disaster victims are more likely to prefer candidates who can improve their living conditions after a natural catastrophe, a rational calculation about which candidate can enhance their well-being. I expect two kinds of candidates to be rewarded after natural disasters based on victims' attempts to reduce the gap between their standard of living before and after the negative shock: those who are associated with the provision of relief and/or social benefits (i.e., welfare candidates), and those who provide signals that they will competently handle the consequences of the shock (i.e., managerial candidates).

The combination of the conjoint and natural experiments shows that victims reward the first type of candidate described. In particular, having experienced material damage from the flood increases the likelihood that a voter will prefer left-wing candidates over those from the right and center by 12 percentage points. This finding is consistent with the idea

that citizens affected by natural disasters seek to improve their living conditions, which leads them to prioritize social policies after the disaster (for example, new housing),[2] and therefore be more likely to vote for the left-wing candidates associated with such measures. Survey evidence from Chile shows that a majority of respondents link social policies, such as public housing, with left-wing politicians (Visconti, 2018), then this ideological label can work as a meaningful heuristic in this context.

This chapter provides two main contributions to the existing literature. First, it investigates a previously overlooked research question about what candidate characteristics become more important to voters after natural disasters. Though the selection of a good type of political leader is a critical component of voters' electoral choices (Fearon, 1999), previous research has focused on traditional sanctioning arguments based on incumbent performance. This chapter, in contrast, stresses the importance of voters' living conditions, and how disaster victims select candidates who can improve them. This logic is not limited to natural disasters, but rather can be applied to other types of negative events. Second, the main findings provide novel insight into how disaster victims make electoral choices. Building upon previous research that has shown that good incumbents are not always punished after disasters (Gasper and Reeves, 2011; Healy and Malhotra, 2010), the results indicate that left-wing candidates have a natural advantage after such events.

The empirical strategy follows a design-based approach to causal inference (i.e., the combination of natural and conjoint experiments), qualitative interviews to illuminate the causal mechanisms at work, the implementation of a behavioral benchmark to compare the findings from the conjoint experiment with the real electoral results after the flood, and the use of survey data from another disaster in a different region in Chile to improve external validity (see Appendix A). The study was registered at Evidence in Governance and Politics prior to the initiation of any research activities (see Appendix B).

[2]Similarly, there is evidence that unemployment increases citizens' support for social policies (Margalit, 2013).

1.2 Theoretical Framework

The Evaluation of Incumbent Performance

Research about how natural disasters affect voters' electoral and political choices has increased in recent years.[3] Most of this literature focuses on voter evaluation of the incumbent based on a process of (mis)attribution of responsibilities (Achen and Bartels, 2016; Bechtel and Hainmueller, 2011; Gasper and Reeves, 2011; Healy and Malhotra, 2009, 2010; Lazarev et al., 2014; Remmer, 2014), or on factors that blur the attribution of responsibility after disasters (Arceneaux and Stein, 2006; Atkeson and Maestas, 2012; Gomez and Wilson, 2008; Maestas et al., 2008; Malhotra and Kuo, 2008).[4]

There are two main arguments in the literature about attribution of responsibilities after natural disasters. The first holds that voters are myopic. For instance, Achen and Bartels, 2016 argue that voters will punish the government during hard times regardless of its ideological platform or performance. Studying the electoral consequences of floods, droughts, and shark attacks in the United States, the authors find that the electorate holds incumbents responsible even for calamities beyond their control. They hold "that voters simply punish incumbent leaders any time their own well-being falls below 'normal' levels, regardless of whether the incumbents have performed well or badly" (ibid., p.138).

The second argument posits that voters reward or punish incumbents depending on their performance handling the consequences of the disaster. For example, Healy and Malhotra, 2010 estimate the effects of exogenous economic losses on electoral outcomes. They find that after tornadoes, voters will punish the incumbent only when no disaster declaration has been made. Therefore, voting behavior in adverse conditions seems to judge competence, rather than being a process of irrational blaming. As Healy and Malhotra (ibid., p.195) hold, "observing that incumbents are adversely affected by natural disasters does not nec-

[3] See Oliver and Reeves, 2015 for an overview of the research on the politics of disaster relief.

[4] There is also a group of articles that study how natural disasters affect turnout (Chen, 2013; Gomez, Hansford, and Krause, 2007; Lasala-Blanco, Shapiro, and Rivera-Burgos, 2017; Sinclair, Hall, and Alvarez, 2011) and political attitudes (Abney and Hill, 1966; Carlin, Love, and Zechmeister, 2014; Fair et al., 2013; Kosec and Mo, 2015; Maldonado, Kronmüller, and Gutierrez, 2016).

essarily mean that voters are irrational. Even though government cannot be blamed for the adverse natural events themselves, they can be held responsible for mitigation, response, and recovery."

The Role of Candidates' Characteristics

Traditional sanctioning arguments, however, only tell one part of the story of how disaster victims make electoral choices. For instance, when the incumbent poorly handles the disaster, we might expect voters to punish them and select another candidate from among the pool of challengers, but we do not know which candidate will be more likely to be elected. In this case, sanctioning arguments do not allow us to infer which challenger will be selected by disaster victims. In Latin America, where all the countries have multiparty competition (i.e., more than one challenger), this last point is particularly important. Thus, in contrast with previous studies, in this chapter I focus on the candidate characteristics that become more important to voters after a natural disaster.

What kind of leaders is the electorate looking for after a natural disaster? I argue that affected voters' choices are driven by instrumental motivations generated by the material damage caused by natural disasters. In particular, disaster victims will make rational decisions about which leader will improve their standard of living.

Affected citizens' instrumental decisions are motivated by new concerns after a natural catastrophe. This reordering of personal priorities and goals implies a reassessment of voters' electoral choices. Victims will make political decisions based on the expected benefits they will receive. As a result, when facing adverse conditions, citizens will select candidates they perceive as more qualified to provide them what they need. That association can be done "without requiring the (probably heroic) assumption that voters actively seek out and process policy-relevant information" (Kim and Margalit, 2017, p.6), because citizens can draw on informational cues and heuristics to make simple connections between policy outcomes and candidate characteristics (Hamill, Lodge, and Blake, 1985; Lau and Redlawsk, 2001).

This hypothesis aligns with what we know about voter decision-making in adverse circumstances. There is evidence that voters try, in times of anxiety, to collect information

in order to decrease their own distress, and then use this new information to make decisions (Marcus, Neuman, and MacKuen, 2000).[5] In consequence, voters should be able to make rational decisions after a natural disaster.

According to my argument, I expect two different kinds of candidates to be rewarded after natural disasters based on victims' attempts to improve their living conditions. The first profile is the "*Welfare Candidate*," a politician that gives rise to expectations of future distribution of welfare. However, welfare can have both a non-programmatic and a programmatic dimension: for instance, a candidate can provide financial relief (non-programmatic) and/or pass social policies (programmatic). Regarding the non-programmatic dimension, affected voters will prefer candidates who send strong signals about the distribution of financial aid, which can help victims buy food and recover some of their essential belongings. Regarding the second dimension, social policies, such as new housing, become crucial for victims, resulting in their greater likelihood of voting for candidates associated with these policies. The proxy used to identify these types of candidates may be nationally specific: for example, in the case of Chile, left-wing candidates are associated with social policies.

It is important to stress that disaster aid (non-programmatic dimension) is not the same as social policies (programmatic dimension). Financial relief, such as the distribution of food baskets, is commonly delivered after natural disasters by NGOs, private actors, and the government, regardless of its ideological affiliation. Social policies, in contrast, fall mainly under the purview of the state, and can be associated with particular parties or ideologies: usually, left-wing parties or candidates. These policies include, for example, the provision of public housing.

How do victims connect expectations about the distribution of disaster relief and promotion of social policies with particular candidates? These expectations can be explained, first, by credible promises made by candidates during the campaign or by previous interactions with the candidates. In other cases, ideological labels may help link candidates to expected social policies. In contexts in which this informational cue may be irrele-

[5]These findings have also been used to understand how voters react to terrorist attacks (Merolla and Zechmeister, 2009).

vant, party labels can work as alternative heuristics (Popkin, 1991).[6] Regardless of the mechanism used to draw these connections, I hypothesize that candidates who generate expectations about the distribution of financial relief (the non-programmatic dimension of welfare) and social policies[7] (the programmatic dimension of welfare) will be favored after natural disasters.

The second profile is the "*Managerial Candidate*." This is a politician who signals that they will competently handle the negative consequences of a disaster. The strength of this type of candidate is based on the idea that a negative event can modify the salience of certain valence issues for affected citizens. A valence issue is one on which all voters hold the same position (Stokes, 1963): for instance, that everyone wants more security, growth, and jobs. In the case of natural disasters, one such issue is that everyone wants a leader competent enough to handle the crisis. Also, valence issues can become more or less salient based on the specific context (Bélanger and Meguid, 2008).

The electorate might use specific candidate characteristics as a proxy for competence on particular issues and select the politician who better fits with the newly salient problem. Certain candidate characteristics can provide information about their capacity to mitigate the effects of a natural disaster: for example, age and education can serve as proxies for managerial competence. Thus, I hypothesize that candidates with more education and experience will be rewarded in adverse circumstances. For example, I expect voters to be more inclined to vote for an old engineer than a young gardener because the former can be associated with the skills necessary for managing a crisis.

The main findings provide evidence that victims support "welfare" candidates, but no evidence that they reward "managerial" candidates. This represents novel evidence about how voters modify their electoral choices after natural catastrophes, and what kind of leaders they are looking for to handle the effects of disasters.

[6] In places where ideological and party labels are meaningless, voters can use other candidate characteristics, such as socioeconomic background, to connect them with social policies or disaster relief.

[7] When ideology is a meaningful heuristic.

Empathic Feelings

It is possible that unexposed citizens also modify their political behavior in response to the catastrophe. This spillover effect could be explained by the existence of empathic or altruistic feelings among unexposed citizens, upon witnessing their neighbors' suffering.

I find evidence that the voting behavior of unexposed voters supports the idea that these individuals may feel empathy toward their victim neighbors. They are highly likely to vote for candidates who generate expectations of disaster relief (the non-programmatic dimension of welfare candidates), and in fact are no different than victims in that regard. Unlike disaster victims, however, they are not more likely to vote for left-wing politicians (the programmatic dimension of welfare candidates). Qualitative evidence from interviews confirms that unexposed citizens feel empathy toward victims.

1.3 Research Design

The 2015 Atacama Floods

The Atacama Desert in northern Chile is one of the driest regions in the world. On March 25, 2015, thunderstorms brought the equivalent of 7 years of rain to the desert in only a few hours, which caused massive flooding in several cities in northern Chile. The terrain in this region is "hard and rocky because rainfall is not frequent or abundant enough for either weathering rocks into sand or supporting the kind of ecosystem that would help turn rocks and minerals into soil. Without soil and plant cover to help absorb rainfall, it just runs off instantly as torrents of water."[8] The floods and mudslides left two dozen people dead and more than a hundred missing, and the government estimated the damage as totaling at least $1.5 billion.[9] More than 30,000 people were affected by the floods, and 3,000 had to live in emergency shelters.[10] As the deputy interior minister declared, this was

[8]The Associated Press, "Thunderstorms Soak Chile Desert in Years of Rain and Kill at Least 9", The Weather Channel, March 27th, 2015.

[9]Taylor, Alan, "Devastating Floods Hit Northern Chile", The Atlantic, April 8th, 2015.

[10]Ford, Dana, "Chile floods: 25 dead, more than 100 missing", CNN, April 25th, 2015.

"the worst rain disaster to fall on the north in 80 years."[11] One of the most devastated areas was Copiapó, the capital city of the Atacama region. Within Copiapó, the most affected area was Paipote, where the mudslides from the mountains entered the city. Even though Paipote was the most damaged locality in Copiapó, some houses were not exposed to the flooding at all.

The floods came from the Andes, following a ravine that was connected downstream with the Copiapó River. However, a small bridge in Paipote stopped the water that was coming from the mountains. A mudslide brought debris, garbage, and sediment that blocked the circulation of water under the bridge. As a consequence, the ravine overflowed, generating damage in many (but not all) areas of the city (see pictures of the bridge and the ravine in Appendix C).

The difference between the more and the less affected areas was that in the former the water flooded houses and generated massive material damage. People living in the most affected sectors lost their homes (and had to live in emergency housing) and their belongings. People living in the less affected areas were isolated for a number of days and suffered from a scarcity of food and supplies. In those areas, there was only a small amount of water in the streets, and it did not enter the houses.

Chile provides a meaningful opportunity to learn about the consequences of natural disasters, because these are common negative shocks (Hewitt, 2014). In addition, the country has stable patterns of programmatic political competition (Roberts, 2013).[12] Voters therefore should be able to connect candidate characteristics with simple policy outcomes. The research design attempts to address each of the three problems presented in the introduction. First, I use a natural experiment where the treatment as a haphazard nature. Second, I focus on a homogeneous town to increase comparability between units and reduce sensitivity to hidden biases. Third, I implement a conjoint experiment to rule out the role of parties nominating particular candidates in the affected districts.

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[11] Staff and agencies in Santiago,"Floods swamp Chile's Atacama region", The Guardian, March 26th, 2015.

[12] The center-left parties are liberal and more pro-state, while the center-right parties are more socially conservative and pro-market. However, the differences on the state-market divide have shrunk over time (Luna, 2014).

Natural Experiment

A natural experiment is a real-world phenomenon that generates haphazard or as-if random assignment to treatment groups (Dunning, 2012; Rosenbaum, 2010). In other words, a particular and rare circumstance generates a situation where some people are exposed to the treatment but others are not, and none of these individuals can predict their future treatment status. The units cannot self-select themselves into the treatment or control groups; and pretreatment covariates should be, in expectation, similar across both groups (Keele and Titiunik, 2016).[13]

In the case of Paipote, the treatment corresponds to the existence of material damage to people's houses. I define as "more affected areas" the sectors where water entered the houses and people therefore suffered material damage due to the flood. I define as "less affected areas" the sectors where the flood did not enter houses and the citizens were only indirectly affected.[14]

The overflow of Paipote's ravine has two main elements that make it possible to define this situation as a natural experiment. First, the magnitude and trajectory of the flood were unpredictable; interviews show that people were not aware of the potential consequences of the rainfall the day before the disaster. Second, people were not aware of the possible negative externalities of the Paipote Bridge, because this was the largest flood in the region in 80 years and a situation like it had never happened before. Therefore, because the disaster and its consequences (due to the bridge) were not anticipated, one would not expect people to have selected their houses based on their expectations of a future natural disaster. This a critical issue because sorting is one of the main threats to any natural experiment.

The interviews help reconstruct the night of the floods, demonstrating that people living in Paipote were not able to predict which areas would be exposed. The story of Carmen, a 21-year-old mother, is a good example of the two points mentioned above.[15] Carmen

[13]The natural intervention should produce independence between treatment assignment and potential outcomes (Keele, 2015).

[14]I determined if an area was more or less affected using qualitative evidence from fieldwork. This decision is confirmed by official government images (figure 1.1), a map marked by the local fire department after the flood (figure 1.2), and satellite images (Appendix D).

[15]The names of the interviewees have been changed according to the IRB consent form, but the age,

lived in an unexposed area where the flood did not enter her house. On the night of the flood she heard firefighters in the streets saying that people needed to evacuate because that area would be affected by mudslides. She decided to go with her baby to her grandparents' house located near the bridge. After a few hours her new refuge was completely flooded, and they barely escaped. Her own house, however, was not affected at all since it was located in an area where water did not enter homes. The decision to move from an unexposed to an exposed area reflects the lack of information about the possible trajectory of the flood (I discuss concerns about spillovers in the next subsection).

The first map shows the more and the less affected areas, the bridge, and the floods coming from the Andes. The second map, created by the local fire department, highlights the flooded areas in red. As expected, the haphazard treatment assignment produced balance in the placebo covariates in the survey, as I show in the results section.

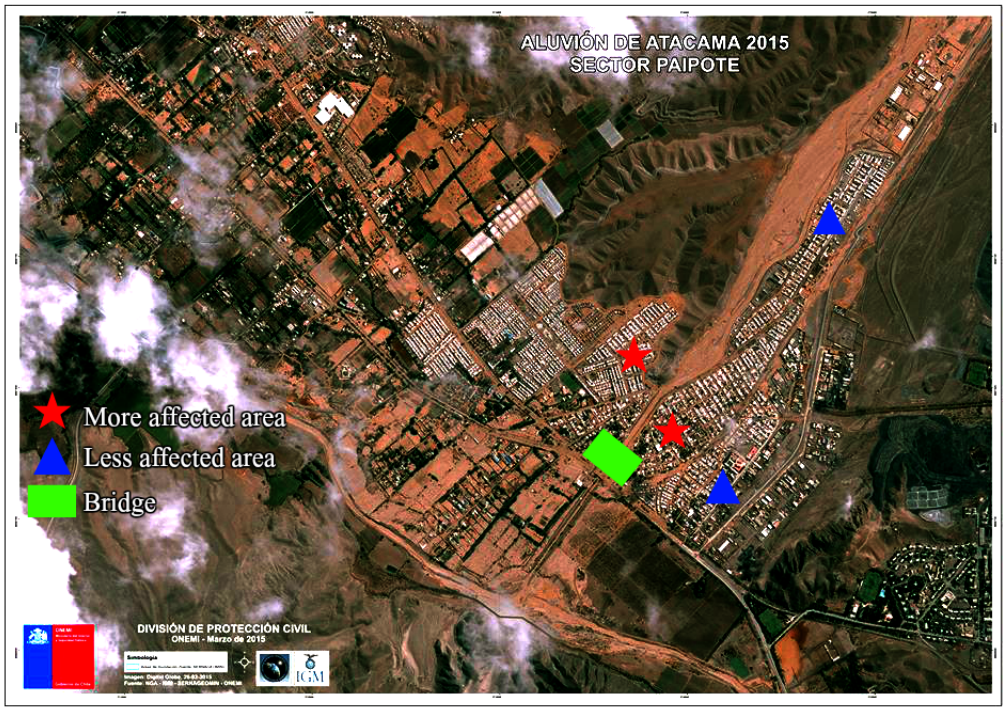


Figure 1.1: Map of Paipote

gender, and occupation (when reported) have not been modified.

Figure 1.2: Map of the affected areas (in red) marked by the local fire department

Spillovers

In natural experiments, the identification of causal effects relies on two core (untestable) assumptions. The first is geographic treatment ignorability (Keele and Titiunik, 2016), which means that the distribution of potential outcomes should be the same for the control and exposed areas. The second is non-interference, or in other words, potential outcomes for any subject do not vary with the treatment assigned to other subjects.

However, as described in the theoretical background, unexposed citizens might present empathic feelings, which could be understood as a spillover effect. Non-victims observe how their neighbors were affected, and they might change their preferences based on that experience. Consequently, a finding of no difference between the groups could have two main interpretations: there are no treatment effects or there are spillover effects. How can we differentiate between a null result and a spillover effect? It is impossible to fully distinguish one from the other, but there are some hints that can help us. For example, it is important to inspect the results within each subgroup and provide qualitative evidence to better understand how exposed and unexposed citizens are modifying their electoral

choices.

On the contrary, if we do find a difference between the groups, that can also have two main meanings: exposed citizens are changing their preferences more than unexposed people or the groups are altering preferences in opposite directions. Qualitative evidence helps us rule out the second alternative because non-victims have empathic feelings toward affected citizens (see section 1.6 for details), and therefore both groups should move toward the same direction. As a consequence, the violation of non-interference assumption should tend to bias the effects towards zero; therefore, any effect can be seen as a conservative estimate (Keele, Titiunik, and Zubizarreta, 2015). In other words, any significant result can be seen as strong evidence of a treatment effect because this is a hard case for finding any result at all.

Reducing Sensitivity to Hidden Biases

Comparing units from the same natural block is desirable in observational studies because unmeasured covariates may be more similar within the block (Pimentel, Kelz, Silber, and Rosenbaum, 2015). Paipote is a homogeneous low-middle income town -for example, 90% of the survey respondents do not have any higher education- which makes the more and the less affected citizens comparable because they are drawn from the same "natural block." Any additional data that increases heterogeneity can also increase bias (Keele, 2015). Rosenbaum, 2005 shows that reducing unit heterogeneity decreases sensitivity to unmeasured biases. In particular, when there is less unit heterogeneity, there needs to be larger unmeasured biases to explain away a given effect (Sekhon, 2009). This benefit cannot be achieved by merely increasing the sample size. Therefore, having a homogeneous sample will improve the comparability between groups of people, and also reduce the sensitivity to hidden biases. As Keele (2015, p.325) summarizes: "there are reasons for focusing on small samples where differences across treated and control units are reduced not by statistical means but by the design" (see Appendix E for an extra strategy to reduce sensitivity to hidden biases).

The Conjoint Experiment

Three months after the floods, I conducted a survey in Paipote with a conjoint experiment embedded in it. The sampling strategy was exactly the same across the more and less affected areas. Streets were selected following a random walk. On a given street, all households were invited to participate in the survey. By the end of the survey, almost the entire town was accounted for.[16] Nine months after the flood, I interviewed 30 individuals from the same area to illuminate the causal mechanisms behind the results.[17] (See Appendix F for more details about the survey implementation.)

I use a conjoint experiment that simultaneously tests the influence of various candidate attributes on respondents' mayoral preferences. The survey experiment asked a sample of Paipote residents to decide between two hypothetical candidates running for mayor in the 2016 local elections (see Appendix G for a discussion about why I use local instead of national elections). The respondents saw information about six attributes of these two candidates: ideological position, gender, previous political experience, profession, age, and proposals for affected citizens (e.g., expectations for financial relief). These characteristics randomly varied across pairings. The outcome was the answer to the following question: if you had to vote for one of these two mayoral candidates, which would you choose? Each of the respondents evaluated eight pairs of profiles. In the analysis I cluster the standard errors by respondent.

I conducted 210 surveys, half in the more affected area of Paipote. Since each respondent rated eight pairs of candidates, and each pair provides two outcomes (a 1 for the preferred candidate and a 0 for the non-preferred candidate), this led to 3360 observations. Following Hainmueller and Hopkins (2015), I also randomly assign the order of the attributes to rule out primacy effects for each respondent. Based on the theoretical expectations, affected citizens should reward left-wing candidates and candidates who generate expectations of the distribution of relief (welfare candidates). They should also be more likely to vote for older and more educated candidates (managerial candidates).

[16] Only one neighborhood was not included in the design, because it was both partially affected and a relatively new area, it could introduce unwanted heterogeneity.

[17] 17 exposed and 13 unexposed citizens.

The following tables summarize the attributes used to generate profiles, and provide an example of a possible pair of profiles evaluated by a respondent. Attributes in both bold and italic represent the candidate characteristics that should be rewarded in comparison to the benchmark category (the first value for each attribute) according to the theory presented in section 1.2. Welfare candidates are represented by ideology (i.e., left-wing politicians) and expectations of financial relief. Managerial candidates are described by age and education. The rest of the attributes help depict a more realistic candidate.

Table 1.1: Profile of candidates

Attributes	**Values**
Ideology	Right
	Center
	Independent
	Left
Profession	Gardener
	Teacher
	Engineer
Gender	Male
	Female
Age	30
	40
	50
Previous Political Experience	No experience
	Council Member
	Mayor
Proposal for affected citizens	Will NOT distribute a financial relief
	Will distribute a financial relief

Table 1.2: Example of experimental design

Attributes	**Candidate 1**	**Candidate 2**
Ideology	Left	Right
Gender	Female	Male
Previous Political Experience	No experience	Council Member
Profession	Gardener	Engineer
Age	30	50
Proposal for affected citizens	Will NOT distribute a financial relief	Will distribute a financial relief

Given that the attribute values were randomized, the design allows us to identify the

effect of each attribute on the probability of being preferred as mayor.[18] This can be estimated by regressing the binary outcome (preferred or non-preferred) on the set of attributes for each profile.[19]

In this chapter, I mainly focus on the interactions between candidate attributes and treatment status to identify how the damage produced by the flood affected the way people make electoral decisions. I compare the electoral choices of citizens who suffered material damage from the flood with those of citizens who did not. Equation 1.1 describes the main quantity of interest:

$$Y = \alpha + \beta_1 Ideology + \beta_2 Profession + \beta_3 Gender + \beta_4 Age + \beta_5 Experience + \beta_6 Expectations + \gamma Treatment + \delta_1 Ideology * Treatment + \delta_2 Profession * Treatment + \delta_3 Gender * Treatment + \delta_4 Age * Treatment + \delta_5 Experience * Treatment + \delta_6 Expectations * Treatment + \varepsilon \quad (1.1)$$

Y represents the candidate selected by the respondents. The coefficients β and δ are vectors, because each attribute contains different values. For example, ideology has four values, but the β vector provides only three coefficients because right-wing candidates are the reference category. The coefficient vectors β_1, β_2, β_3, β_4, β_5 and β_6 describe the effect of the candidates' attributes on the control group. Consequently, the vectors of interest are δ_1, δ_2, δ_3, δ_4, δ_5 and δ_6, because they describe the change in effect of the candidates' attributes between control and exposed conditions.

Defining the Treatment

Half of the surveys and conjoint experiments were conducted in the more affected areas of Paipote. However, some flood victims moved to houses located in the less affected areas

[18] I follow the approach developed by Hainmueller, Hopkins, and Yamamoto, 2014 to estimate the average marginal component effect (AMCE). This represents the average difference in the probability of being preferred as mayor when comparing two different attribute values: for example, a "female" candidate versus a "male" candidate. And due to the random assignment of attributes, the "female" and "male" profiles will have, on average, the same distribution for all the other attributes (Hainmueller and Hopkins, 2015).

[19] The estimator for the AMCE is nonparametric and does not require a functional form assumption (Hainmueller, Hopkins, and Yamamoto, 2014).

to live temporarily with relatives or friends. In particular, seven survey respondents in a less affected area were actually flood victims who lived in a more affected area the night of the disaster. Therefore, 112 respondents lived in the more affected area during the natural disaster, and 98 in the less affected one.

The haphazard nature of the flood generated two different sectors: one where people suffered extensive material damage due to the flood, and another where the mudslides did not enter homes. The following table reports the number of people from these two areas that reported material damage after the flood.[20]

Table 1.3: Exposed and unexposed respondents

	More affected area	Less affected area	Total
Material damage reported	109	4	113
No material damage reported	3	94	97
Total	112	98	210

Material damage status is almost perfectly correlated with the area where the subjects were living. In the analysis the treatment status is equal to 1 if the respondent reported material damage, and 0 if he or she reported indirect or no damage.[21] The results are the same when using the area as the treatment (see Appendix H). The subjects who received the treatment will be referred to, from now on, as the "exposed group," and those that did not report material damage as the "unexposed or control group." Five percent of the survey respondents did not want to participate in the conjoint experiment or quit before finishing it: three in the less affected area and seven in the more affected area. I found no evidence to support the idea that the treatment affected the probability of completing the conjoint

[20]The survey included the following question: How affected were you by the floods? The answers were categorized as follow: 1 when respondents said "nothing happened," 2 when they reported indirect consequences such as isolation, 3 when they reported partial material damage, and 4 when they reported complete material damage. The first and second categories generate the "no material damage" status, and the third and fourth the "material damage" status.

[21]It is possible to imagine that this natural experiment involves assignment to treatment into "hypothetical clusters." However, it is not clear what such a cluster would consist of with this design (a street, a group of streets, a block, a group of blocks, etc.). Additionally, because Paipote is an homogeneous town, I expect the citizens within each "hypothetical cluster" to be no more similar than citizens in other "hypothetical clusters."

experiment (p-value: 0.30).[22] These 10 respondents are excluded from further analysis. Therefore, there are 106 individuals in the exposed group and 94 in the unexposed group, which leads to a total of 3200 observations (16 candidate-pairs evaluated by respondent.)

1.4 Results: Natural and Conjoint Experiment

Covariate Balance

The exposed and unexposed citizens should have similar distributions of observed and unobserved covariates. Although there are no pretreatment covariates available in this study, a number of the variables captured in the survey should not be affected by the treatment (placebo covariates), such as gender,[23] age, and education.[24] The next table reports the means and the standardized differences for the three placebo covariates.

Table 1.4: Balance of placebo covariates

Covariate	Mean exposed	Mean control	Standardized difference
Gender	1.72	1.77	0.11
Age	46.21	43.41	0.19
Education	3.20	3.01	0.14

Both groups are comparable because their standardized differences are below 0.2. One-fifth of a standard deviation is the usual rule of thumb for checking if covariate balance was achieved (Silber et al., 2013). It is also possible, however, to improve balance by constraining the standardized differences to be lower than 0.05 using optimal multivariate matching (see Appendix H). This statistical method helps reduce overt biases. Though hidden biases are still a threat in any observational study, the particularities of Paipote

[22] I tested this by regressing a binary indicator of a failed conjoint experiment on the treatment.

[23] Male:1, Female:2.

[24] 1: Primary Education Incomplete, 2: Primary Education Complete, 3: Secondary Education Incomplete, 4: Secondary Education Complete, 5: College Education Incomplete, 6: College Education Complete, 7: Graduate Studies.

(specifically its being a homogeneous residential town) and the haphazard nature of the treatment assignment makes the comparison between these groups more credible.

Voters' Electoral Choices

Figure 1.3 provides a graphical comparison of the electoral choices of exposed and unexposed respondents. Based on the theoretical expectations, affected citizens should be more likely to vote for welfare and managerial candidates.

The plots on the left provide the β coefficient vectors for each subgroup of citizens. The plot on the right displays the interaction results from equation 1 or, in other words, the differences between the control and exposed groups (δ coefficient vectors). These results are interpreted as the effects of the flood on the attributes that explain the probability of being preferred as mayor. The dots indicate point estimates, and the lines indicate 95% confidence intervals. The reference categories are the dots without confidence intervals (the first category for each attribute).

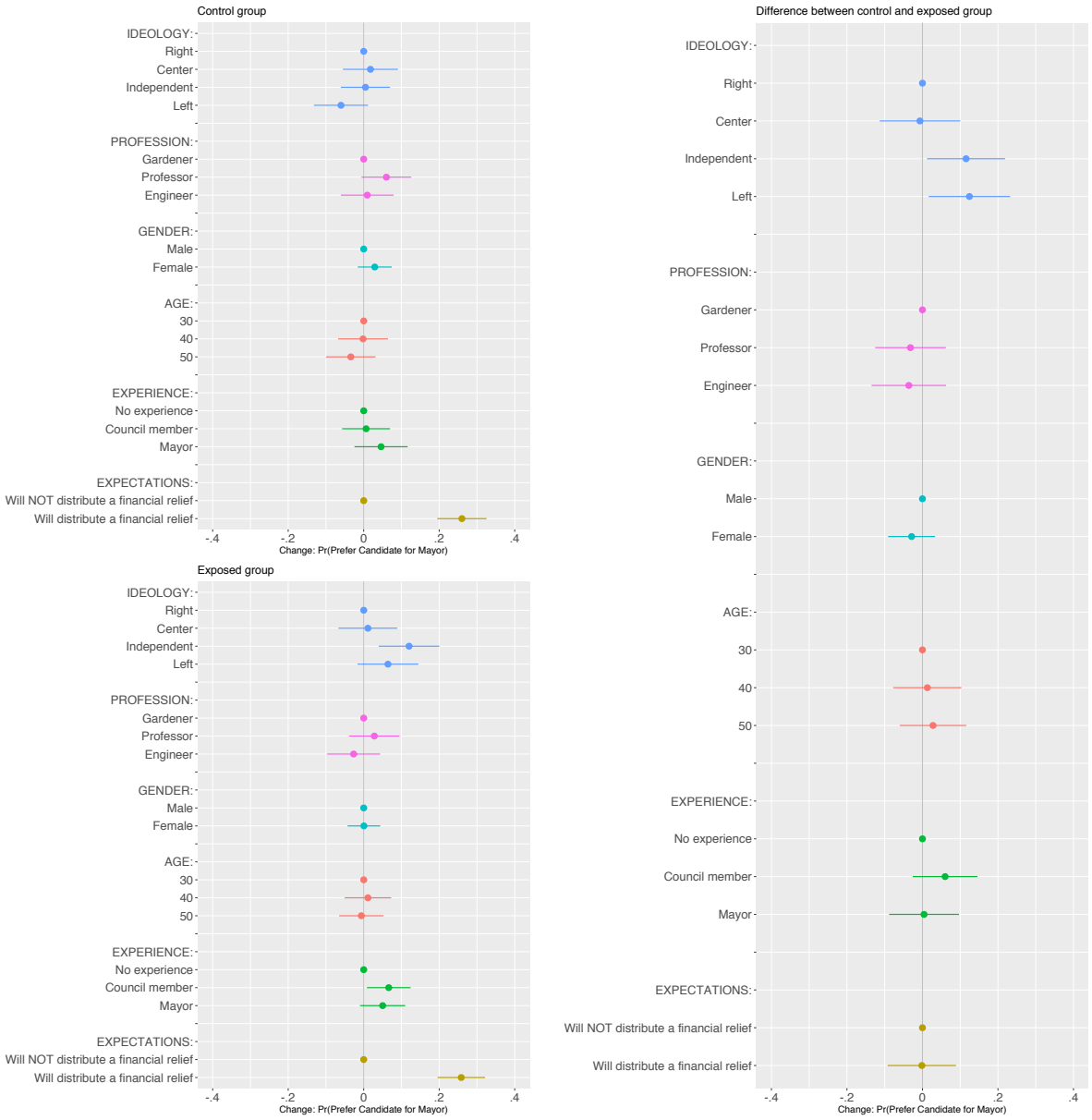


Figure 1.3: Effects of candidates' attributes on probability of being voted for mayor

Affected and unaffected citizens do have different ideological choices. Independent and left-wing candidates become more attractive for disaster victims. The difference plot shows that flood exposure increases the chances of preferring a left-wing candidate over a right-wing candidate by 12 percentage points. Material damage due to the flood also increases the probability of preferring a left-wing over a centrist candidate by 12 percentage points (the full regression with the β coefficients is displayed in Appendix I).

Why are disaster victims more likely to vote for left-wing candidates? There are multiple answers to this question, and the conjoint experiment is not enough to understand the mechanisms involved. One response is that voters associate left-wing candidates with the mayor or the opposition and they are rewarding/punishing real politicians by using ideology as a proxy. A second option is that they prefer left-wing politicians for the policies they can implement. I conducted interviews and provide extra survey evidence to support this former point (I discuss the first option, and more alternative hypotheses, in Appendix J).

Independent candidates also have an electoral advantage in exposed areas, although not over left-wing candidates (see Appendix K). Natural disasters might also modify victims' political attitudes (Carlin, Love, and Zechmeister, 2014; Fair et al., 2013). Consequently, the advantage of independent candidates versus right-wing or centrist ones can be an expression of voters' new attitudes toward the political system. There are similar findings in the economic voting literature in Latin America, where negative economic conditions have been associated with the deterioration of traditional parties' vote share (Carreras, 2012; Lupu, 2014; Murillo and Visconti, 2017). Therefore, natural disasters might have a similar effect on affected voters, making them more likely to support independent candidates.

However, there is also evidence of voters' empathic feelings in their electoral decisions. Both exposed and unexposed citizens are highly likely to prefer candidates who want to distribute financial relief to disaster victims, even though unexposed respondents were not affected.[25]

Why would victims and non-victims have similar preferences regarding the distribution of short-term benefits? This is not a pure null result because this characteristic is the most important factor explaining voters' decisions in each subgroup, but there is no difference between the exposed group and the control. This is congruent with a spillover hypothesis. Non-victims display empathic feelings towards their neighbors because they are seeing them suffer. Qualitative evidence supports this argument. There are no reasons to believe that the other attributes that report null results within each subgroup and between

[25] An alternative option is that both groups had the same preference regarding the distribution of short-term benefits before the natural disaster, and material damage due to the flood did not change those preferences. That option seems very unlikely based on the magnitude of the catastrophe.

the subgroups (e.g., gender) are evidence of spillover effects.

Finally, there is no evidence that managerial characteristics are important to voters. They are not more likely to vote for older or more educated candidates, and there are no differences between the groups. The interviews are a useful tool for understanding these null effects. Victims strongly focus on the distribution of welfare and relief, which overcomes the importance of other factors that might also be important for citizens, such as selecting politicians with more experience or expertise.

1.5 Behavioral Benchmark

The most relevant critique of conjoint experiments is that participants are evaluating hypothetical choices; in real life they might be making different decisions. Following Hainmueller, Hangartner, and Yamamoto, 2015 approach, one method of validating the conjoint analysis is to compare it with actual voting behavior: citizens' response to the 2015 flood in the 2016 local elections.

In this behavioral benchmark, the outcome is not the incumbent vote share, as it would be in the case of traditional research studying retrospective voting. First, I analyze the impact of the flood on voting for leftist, rightist, centrist, and independent candidates (welfare candidate hypothesis).[26] Second, I analyze the effect of the flood on voting for older and more educated candidates.[27]

How can I compare affected and unaffected areas? The government declared a state of constitutional exception due to the catastrophe in 11 counties, therefore those municipalities are defined as the exposed units. One empirical strategy is to select 11 unaffected counties that are similar to the exposed municipalities. Ideally, the control group should be similar in terms of (i) unobserved and (ii) observed covariates.

Regarding point (i), I restrict the group of eligible control units to counties located

[26]It is not possible to test the role of the expectations about distribution of disaster relief in a behavioral benchmark.

[27]This empirical strategy cannot rule out the role of retrospective accountability. However, it is studying the political consequences of disasters by a different dimension since it focuses on the candidates' characteristics rather than the incumbent vote share.

north of Santiago, the capital city. The idea is to have a natural block of eligible counties from the center-north of Chile, and exclude all the municipalities located in the capital and the south of the country because they might have multiple unobserved characteristics if compared to places in northern Chile.

Regarding point (ii), I select from the sample of eligible units 11 control counties that are similar to the affected municipalities in terms of observed characteristics. I use the following pretreatment covariates to make more credible comparisons: the right, center, left, and independent candidates vote share in the 2012 local election, total population, percentage of rural population, human development index, and poverty levels. These covariates are included because they have been studied as factors explaining voters' behavior in Chile (Altman, 2004; Calvo and Murillo, 2012; González, 1999; López, 2004; Luna, 2010; Navia, Izquierdo, and Morales, 2008).

The control units are obtained using recent advances in mathematical programming (Zubizarreta, Paredes, and Rosenbaum, 2014; Zubizarreta and Kilcioglu, 2016). I use cardinality matching to obtain 11 control units that are similar to the 11 exposed counties. In particular, the goal was to achieve the largest matched sample that reduces the standardized differences in means between the groups (see Appendix L for more details about the covariates and the selection of units).

The following table shows that covariate balance was achieved for all the pretreatment county characteristics. The algorithm kept the 11 affected counties, and optimally selected 11 other municipalities to reduce the standardized differences between both groups. The standardized differences are below the traditional requirements for illustrating balance, one-fifth of a standard deviation (Silber et al., 2013).

Table 1.5: Balance of pretreatment covariates

Covariate	Mean exposed	Mean control	Standardized difference
Left-wing candidates	0.60	0.59	0.05
Right-wing candidates	0.18	0.21	0.18
Centrist candidates	0.07	0.07	0.02
Independent candidates	0.15	0.13	0.11
Total population	53,808	47,016	0.08
Percentage of rural population	0.21	0.23	0.07
Human Development Index	0.72	0.72	0.03
Poverty	0.14	0.13	0.15

I use equation 1.2 to estimate the effect of the flood (disaster declaration) at the county level. The matched sample used for this estimation is not just balanced in terms of observed covariates, but was constructed while attempting to reduce sensitivity to hidden biases by focusing on a natural block to generate credible comparisons (cities to the north of Santiago).

$$Y_c = \alpha + \beta_1 T_c + \sigma_n + \varepsilon_c \qquad (1.2)$$

Y represents the outcome of interest for the 2016 election (vote share of left, right, centrist, independent, more educated,[28] and older candidates.[29] T depicts the treatment (declaration of emergency). σ_n represents region fixed effects. I expect to find results that go in the same direction as the conjoint experiment, but because of power issues they might not be significant (n=22).

[28] 0: High school or less, 1: More than high school. Source: public declaration of patrimony.

[29] 0: less than 50 years old, 1: more than 50 years old.

Table 1.6: Regression results

	Behavioral Benchmark: Welfare Candidates			
	Left	Right	Center	Independent
	(1)	(2)	(3)	(4)
Flood	0.097	−0.360*	−0.063*	0.327
	(0.203)	(0.175)	(0.036)	(0.286)
Region fixed effects	Yes	Yes	Yes	Yes
Observations	22	22	22	22
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 1.7: Regression results

	Behavioral Benchmark: Managerial Candidates	
	Age	Education
	(1)	(2)
Flood	−0.035	0.086
	(0.279)	(0.323)
Region fixed effects	Yes	Yes
Observations	22	22
Note:		*p<0.1; **p<0.05; ***p<0.01

It is important to remember that we cannot directly compare the coefficients of the conjoint experiment with the behavioral benchmarks because the estimates of the former are obtained using a reference category. However, we should pay attention to the size and direction of the estimates. The results show that right-wing and centrist candidates were punished in the affected counties; meanwhile, the estimates for left-wing and independent candidates show a positive but non-significant effect. The large standard errors are probably explained by the small sample size. However, the direction of the coefficients for the

welfare candidate perfectly matches the conjoint experiment. There is a positive correlation between disasters and voting for left-wing and independent candidates, and a negative one between disasters and voting for right-wing and centrist ones. The findings are also congruent for the managerial candidates' characteristics. Citizens do not seem particularly focused on selecting more experienced and educated candidates.

All these results provide more robust evidence about how disaster victims evaluate candidates' ideological labels and increase the external validity of the conjoint analysis. Voters are more likely to vote for candidates associated with social policies (or punish candidates not associated with them), and these preferences seem to overcome any focus on managerial attributes.

1.6 Causal Mechanisms

I interviewed 30 affected and unaffected residents of Paipote to understand the logic behind their electoral choices after the flood (see Appendix M for interviews in Spanish). This supplements the data from the combined conjoined and natural experiment, which though particularly useful for studying the effect of the natural disaster, does not help us understand the causal mechanisms at work. I use direct content analysis to interpret the interviews, an approach based on the use of relevant research findings as guidelines when analyzing the data (Hsieh and Shannon, 2005). The main goal was to provide answers to two questions derived from the conjoint experiment. First, why do left-wing candidates become more attractive to victims? Second, why are both victims and unexposed citizens likely to vote for candidates who want to provide financial benefits to the victims?

Regarding the first question, interview responses reveal victim concern about the material damage inflicted by the flood. Most of them lost their homes or all their belongings. Daniela is a 31-year-old housewife who provides the following account of how the flood changed her life: "I had to change all the projects I had. I had to move backward. A lot of them got cut, and I had to replace them with others. [For example,] fixing my house, because we have not had any help [...]. The priority right now is the house – the other things were pushed to the background." Rosa is a 44-year-old housewife who was emotionally

and materially affected by the disaster: "After the floods everything changed [...]. I had aspirations, I had dreams, and I had to put them on hold [...]. For me it's been hard. My son had to drop out of college, and that has been tough for me too [...]. On March 26th I saw my house full of mud, and I did not know where I would sleep that night. [I thought], tomorrow I'll wake up and everything will be fine, because this was only a dream." These two testimonies illustrate how victims had to focus on new concerns, and how their most critical need was to improve their living conditions by fixing, cleaning, and repairing their houses. The role of the state is crucial in this context: it is the only actor that can shrink the gap between how victims are currently living and how they lived before the disaster.

Affected citizens' new priorities have direct consequences on the policies they most care about. Pedro is a 39-year-old farmer, and he said the following: "It is not just financial relief; we also need more material support. As my brother says, we need fences, houses, a permanent home [...]. The best help would be a house, but we are not asking for a huge house, but something that we can keep improving." Daniela provides more insight into victims' policy preferences: "[We need] solutions to our problems and not stopgap measures [...]. [The government] should focus on the key things and give priority to the issues that have real relevance [. . .]. It is more important to fix a house where a child needs a home to live than a bus stop." These interviews show how victims focus on multidimensional social policies–in particular, on housing–and not on just short-term relief.

These new policy preferences (i.e, focus on housing) will affect victims' electoral choices. Manuel is a 30-year-old miner. When he was asked about what kind of candidate he would prefer for the locality, he responded: "I think that when one chooses someone, it is not because of the distribution of short-term benefits, but because of a more general commitment to the community [...]. Who benefited from a two or three *luca*[30] handout? No one in the long run. We need something concrete because if I provide short-term aid, nothing will improve for the people. We need permanent, and not temporary, solutions." Claudia, a 23-year-old teacher, has a similar opinion about the ideal candidate for Copiapó: "I would like the next mayor to focus on people's quality of life [...], in every aspect, not just in that they give me a food basket, but in other ways too." Therefore, it is possible to

[30]Two *lucas* are two thousand pesos or three US dollars.

expect that candidates associated with social policies should be rewarded in this specific context.

Regarding the second question about why both groups have similar preferences about candidates that will distribute disaster aid, the interviews show that unexposed citizens are motivated in part by empathy toward victims. Throughout our conversations, unexposed citizens constantly cited examples of their neighbors' suffering, indicating their empathy towards them. For example, Ana is a 33-year-old housewife who was not exposed to the flood. She mentions how difficult it was for her "to hear the testimony of the people, to hear how they survived, how [some of them] had to tie themselves to a fence so the water did not sweep them away [...] and how some kids lost everything." Tania is a 40-year-old housewife and also a non-victim. She provides the following anecdote: "I remember that when I was on the bus, I met a couple of grandparents who were going to the store. I helped them to walk back to their house, and the grandmother told me she'd lost everything, and her daughter lives with them, but only the daughter got relief benefits. What do you think about that – if they are two families, they should get two benefits, but got only one?" These interviews provide evidence about how non-victims have empathic feelings towards the victims. This finding can help us to understand why both group of citizens are equally likely to prefer candidates that will distribute disaster aid (non-programmatic benefits). However, those empathic feelings have a limit since victims are more likely to vote for left-wing and independent candidates.

There is a third question critical for understanding the causal mechanisms behind voter preferences. The conjoint experiment shows that affected citizens are more likely than non-affected voters to prefer left-wing candidates. However, can Chileans connect ideological labels with policy ideas? Calvo and Murillo, 2012 show that voters have the capacity to locate the main two coalitions on the left-right ideological spectrum. Additionally, Zeichmeister, 2015 shows that in Chile and 12 other Latin American countries, left-right self-placement is a significant predictor of the left-right vote. The argument of this chapter can also be applied to countries where ideology is not as relevant as in Chile. Voters only need a very basic understanding of the political system to be able to link simple policy ideas (or outcomes) with party labels or other candidate characteristics.

The evidence from this chapter shows that voters can rely on candidates' ideological labels when voting. It is important to stress that this is not the same as traditional ideological voting. The latter assumes that voters are able to place themselves and candidates on an ideological scale and then minimize the distance between their own position and the favored candidate's position on the spectrum. On the contrary, I assume that voters can use the information contained in candidates' ideological labels to make electoral decisions, not that they are always minimizing distances. Affected voters are more likely to select left-wing candidates because of what those candidates represent and not because victims moved to the left on the ideological spectrum.

1.7 Conclusions

Voters living in developing countries are frequently exposed to natural disasters and negative income shocks, where a lack of preparedness and lower state capacity make them very vulnerable to negative events. These voters may be even more exposed to catastrophes as global warming intensifies. Climate scientists are increasingly concerned that rising temperatures will step up the intensity and frequency of natural disasters. As the general increase in temperature has resulted in a rise in the number of hot days, warmer air fosters the evaporation of water, which may cause more intense rainfalls and snow events, which can contribute to an increasing risk of certain types of natural disasters (Lippsett, 2012; Zseleczky and Yosef, 2014). These events, in turn, may contribute to a greater saliency of the politics of natural disaster.

This research provides a novel finding about voter reactions to natural disasters: victims are more likely to focus on the distribution of social benefits such as new housing, and as a consequence are more likely to vote for candidates associated with those policies (left-wing politicians in the case of Chile). Though external validity could be a concern because the primary evidence comes from one particular place, respondent characteristics (i.e., low-middle income and educational levels) accurately represent the median voter in Latin America, and experimental results are paired with real electoral outcomes. In addition, evidence from a different natural disaster in a different region of Chile points in the

same direction (see Appendix A).

An important challenge to address is that even though natural disasters might affect an area without deliberately targeting it, they are not randomized experiments. Nevertheless, natural experiments within natural blocks provide an opportunity to address this issue because treatment assignment has an as-if random nature due to certain unusual circumstances and homogeneous units should have more similar unmeasured covariates. I exploit the haphazard nature of the 2015 floods in Paipote, and the town's high level of homogeneity, to understand how adverse conditions affect voters' ideological choices.

The conjoint experiment shows that the treatment (material damage due to the flood) increases the probability of preferring left-wing candidates. Qualitative interviews help us understand that victims focus on multidimensional solutions to improve their living conditions and consider social policies to be the most important path toward recovery from the disaster. Therefore, left-wing candidates should have a natural advantage over right-wing politicians because the former can be linked to the policies victims would like to see implemented. In addition, unaffected voters exhibit empathy toward victims when making electoral decisions. This finding should be taken into account when studying the consequences of natural disasters.

The argument of this chapter can also be applied in countries where ideology does not explain voter behavior, such as Brazil. In that particular case, however, I would expect voters to link the distribution of social policies with the PT (Workers' Party). In consequence, that party should hold an advantage over other political parties after natural disasters.

The floods in northern Chile help us learn about how disaster victims tend to reward candidates with certain characteristics. This argument, however, can be extended beyond natural catastrophes to include other types of negative shocks. For example, crime victimization might make right-wing candidates more attractive to voters because they may be more likely to implement victims' new policy priorities, such as iron-fist crime-reduction policies. Disaster or crime victims will not only focus on the incumbent's performance, as previous research has argued, but will also select a political leader who they think will be able to enhance their living conditions after the negative event.

1.8 Appendices

Appendix A: External Validity

The main evidence is coming from a particular natural disaster in the north of Chile. In this section I explore how a different disaster in a different region of the country can produce similar effects.

In 2010, the central-southern regions of Chile were shattered by an earthquake of magnitude 8.8. This was the 4th strongest earthquake the world had experienced during the previous 50 years. I exploit a national survey conducted four months after the flood to understand how this disaster might affect citizens' political preferences.[31] I follow Zubizarreta, Cerdá, and Rosenbaum, 2013 strategy to select affected counties by using the intensity of the earthquake at the county level. Counties with peak ground acceleration greater than 0.275g are identified as exposed. Respondents from those counties are assigned to the treatment group. Meanwhile, participants from municipalities that were not part of the reconstruction plan, and therefore were not affected by the earthquake, are categorized as controls. I find the largest matched sample that achieves covariate balance on three placebo covariates (i.e., gender, age, and education) by using cardinality matching. The following table reports the standardized differences between both groups, which are below 0.2 (Silber et al., 2013).[32]

[31] I use the national representative survey conducted by the Centro de Estudios Públicos (CEP) in June-July 2010.

[32] The affected counties selected by the algorithm are: Arauco, Buin, Bulnes, Cabrero, Casablanca, Cauquenes, Chanco, Chiguayante, Chillan, Chillan Viejo, Concepcion, Constitucion, Coronel, Curanilahue, El Quisco, Graneros, Las Cabras, Linares, Litueche, Los Angeles, Lota, Maria Pinto, Melipilla, Ninhue, Penco, Renaico, Retiro, San Carlos, San Javier, San Pedro de la Paz, San Vicente, Santa Cruz, Talca, and Talcahuano. The control counties selected by the algorithm are: Antofagasta, Arica, Calama, Calbuco, Castro, Copiapo, Coquimbo, Coyhaique, Curaco de Velez, Illapel, Iquique, La Serena, Lago Ranco, Maullin, Natales, Osorno, Ovalle, Paillaco, Panguipulli, Puerto Montt, Punta Arenas, Quemchi, Rio Negro, San Pablo, Tocopilla, Valdivia, and Vallenar.

Table 1.8: Balance of pretreatment covariates

Covariate	Mean exposed	Mean control	Standardized difference
Gender	1.55	1.58	0.07
Age	45.63	45.94	0.02
Education	3.21	3.49	0.15

The survey did not ask about preferences for welfare and social policies. However, the survey included a group of questions that can help us test some implications of the main argument. If victims support the distribution of welfare policies, those measures must be funded from somewhere. As a consequence, it is possible to expect that victims might also be more likely to support a raise in taxes.[33] The survey asked the following question: Do you agree or disagree with the following measures to fund the reconstruction efforts after the earthquake? (1) to raise taxes, and (2) to raise taxes on mining companies.[34] I use equation 1.3 to estimate the effect of the earthquake on victims' preferences regarding taxation. I cluster the standard errors at the municipality level.

$$Y_c = \alpha + \beta_1 T_c + \sigma_n + \varepsilon_c \qquad (1.3)$$

Y represents the outcome of interest (support a raise on taxes for the reconstruction efforts). T depicts the treatment (respondent living in a county affected by the earthquake). σ_n represents region fixed effects. This natural disaster should increase support for these measures because these can be linked to the implementation of welfare policies to improve citizens' living conditions after the earthquake.

[33]This analysis is based on the assumption that Chilean citizens can connect more taxes, welfare policies, and left-wing politicians.)

[34]There are other questions that are less relevant, such as to raise taxes on cigarettes.

Table 1.9: Regression results

	Policy Preferences	
	Mores taxes 1	More taxes 2
	(1)	(2)
Earthquake	0.437***	0.104***
	(0.000)	(0.000)
County fixed effects	Yes	Yes
Observations	478	478
Note:	*p<0.1; **p<0.05; ***p<0.01	

As expected, affected citizens are more likely to support a raise in taxes. The framing of the question directly links the taxes with the reconstruction efforts. Affected citizens have instrumental motivations, mainly based on improving their living conditions, and are more likely to support policies that reduce the gap between how they used to live before the earthquake and their living conditions after the earthquake. This particular post-disaster context provides a natural advantage to left-wing candidates.

Appendix B: Pre-analysis Plan

I pre-registered a preliminary theory and design before any research activity. In the pre-analysis plan I described the characteristics of the conjoint experiment, in particular, the candidates' attributes that would be randomized. The following is an excerpt from the preregistration: "*The experiment will ask a population of citizens living in the city of Copiapo to decide between two (non-real) candidates that will be competing for the position of mayor in the 2016 local elections. The respondents will see information about six attributes of these two candidates: ideology, gender, previous political experience, profession, age and proposal for affected citizens (proxy of distribution). These attributes will be randomly chosen to generate the candidates profiles. This experimental design allows for the comparison of the explanatory power of different treatments (Hainmueller and Hopkins 2014). (...) The outcome will be the answer to the following question: if you have to vote for one of these two candidates, whom do you prefer for mayor? Each of the respondents will have to evaluate 8 pairs of profiles. Therefore, in the analysis it will be necessary to cluster the standard errors by respondent.*"

The preliminary design intended to use flood damage as a covariate instead of a treatment. The pre-analysis plan said: the "*empirical design will allow me to study the interactions between candidates attributes and respondents' characteristics. In particular I will focus on how the damage produced by the floods at the individual level (pretreatment covariate) affects the way people make electoral decisions.*" I learned about the natural experiment in the field. After having this new information, I decided to interpret the results as the treatment effect of flood damage.

In the pre-analysis plan I registered the following preliminary theoretical framework: "*What explains voters' political preferences? There are multiple factors that affect voters' electoral behavior, but these can be aggregated in two main categories (Adams et al. 2005, Calvo and Murillo 2015). The first relies on the role of ideology, and assumes that voters and parties locate themselves along an ideal point on some ideological continuum. Voters prefer the candidate/party that minimizes ideological distance. The second category emphasizes the existence of non-ideological considerations in voters' decision making. This*

may involve voters taking into account some non-policy-related factors when they are deciding to vote for a particular candidate, such as descriptive representation (e.g. race, gender or social class), targeted distribution (e.g. vote-buying or patronage) and retrospective voting (reward/punish the incumbent when economic condition improve/worsen), among other non-programmatic variables. Adams et al. (2015) attempt to reconcile both groups of arguments by proposing a unified model of voting behavior, which integrates the behavioralist's perspective into the spatial-modeling framework. Therefore, the combination of the programmatic and non-programmatic components will explain voters' electoral decisions. However, all the theories that unified the spatial and sociological explanations assume that voters have fixed preferences regardless of the social and economic context. Ideology will have the same importance for voter i when she votes during adverse conditions (e.g. natural disaster or an economic crisis) and normal times. This project challenges this view, arguing that the importance of the ideological and non-ideological determinants of the vote are conditional to the context. Simply put, adverse conditions produced by natural disasters will affect the role of the ideological and non-ideological factors that explain voters' political preferences."

From this framework, I presented three hypotheses: (1) Political preferences are conditional on the magnitude of the negative shock. (2) Ideology (i.e. ideological congruence) will be less relevant to voters' preferences where the damage from the disaster was higher. (3) Future distribution of financial relief will be more important for explaining voters' preferences where the damage from the disaster was higher.

The first hypothesis was confirmed: affected citizens have different political preferences than unexposed citizens, in particular regarding their ideological preferences. The second hypothesis was also confirmed, because ideological congruence is less relevant for exposed citizens. Ideological congruence is the difference between a voter's self-placement in the ideological spectrum and the ideology of her or his preferred candidate. The results show that respondents did not change their ideological placement, but affected citizens are more likely to vote for left-wing politicians. Consequently, ideological congruence becomes less salient for them. The third hypothesis was not confirmed because of the spillover effects discussed in the chapter. This latter discussion was incorporated in the

dissertation after I learned about the empathic feelings in the field.

The previous theoretical framework mainly focused on the role of ideological congruence. Natural disasters do, in fact, reduce the ideological congruence between voters and parties, because the former are willing to vote for new candidates. However, this is a consequence of victims' focus on improving their living conditions. Therefore, the new theoretical framework (i.e., disaster victims are more likely to select political authorities who can increase their well-being after the catastrophe) is taking a step backwards to better understand voters' political preferences after natural disasters. The lack of ideological congruence is now an implication of the main theory.

In summary, I made two main amendments to the pre-analysis plan. First, I re-conceptualized flood damage as a treatment instead of a covariate. The analysis remains the same (interaction between flood damage and the conjoint experiment). Second, the preliminary theory focuses on ideological congruence, but now I develop a more general theory about how citizens modify their political preferences after natural disasters.

Appendix C: The Ravine and the Bridge

Figure 1.4: Paipote's ravine

Figure 1.5: Paipote's bridge

Appendix D: Exposed and Unexposed Areas

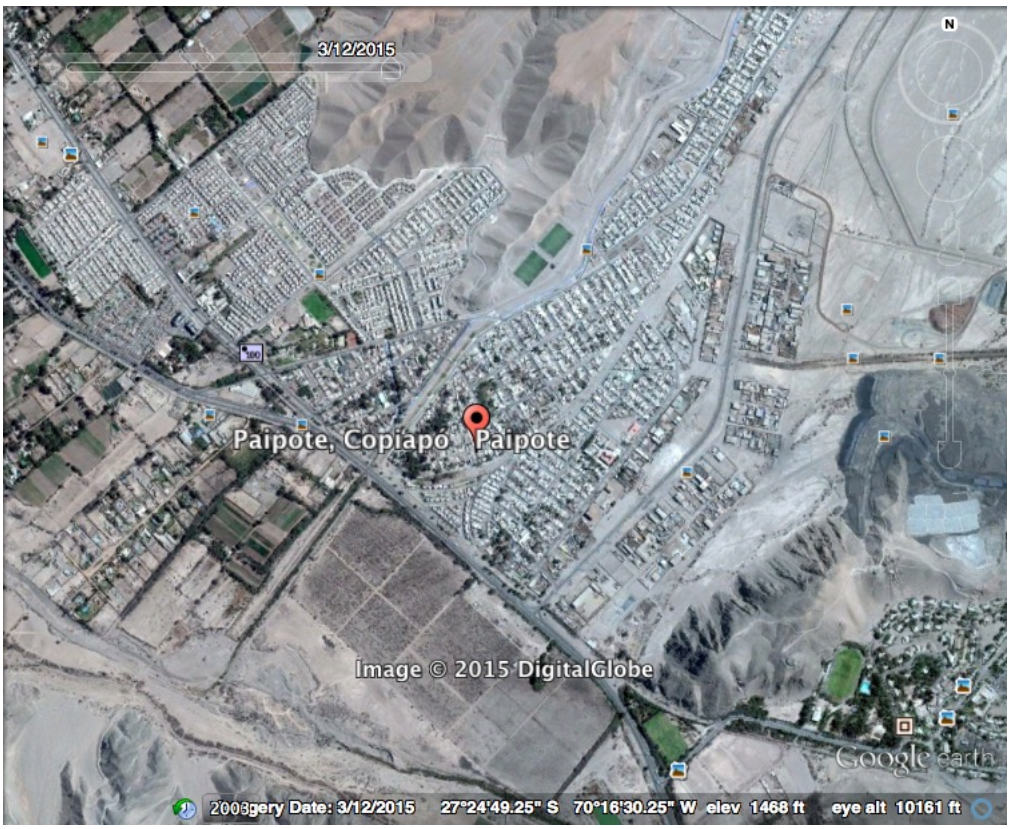


Figure 1.6: Google Earth; before the floods

Figure 1.7: Google Earth; after the floods

Appendix E: Differential Effects

The comparison between the more and the less affected areas of Paipote is similar to the concept of differential effects developed by Rosenbaum, 2006. Differential effects are immune by design to generic unobserved biases, since they should affect different treatment conditions in similar ways. Consequently, it is possible to remove the generic unmeasured biases by studying associated or parallel treatments. For example, if we want to compare the effects of crack cocaine use during pregnancy, a comparison between treated and control subjects is likely to be biased since a woman who uses crack might engage in other unmeasured activities that can also put the fetus at risk. However, it is possible to expect a similar pattern of behavior by a woman who uses marijuana during pregnancy (ibid.). Therefore, the comparison of two treatment conditions, crack cocaine and marijuana, and the exclusion of a pure control group, will allow us to rule out the generic unobserved biases common in both treatments. In the case of Paipote, there are two treatment conditions: being directly and being indirectly affected by the flood. Hence, a pure control group constructed with people from a different city that were not affected (directly or indirectly) by the flood might not be as good a comparison as the unexposed citizens from Paipote.

In summary, this research design exploits two features to decrease sensitivity to hidden biases: the low heterogeneity in Paipote, since both groups are coming from the same natural block, as well as the differential effects generated by the comparison of two associated treatment conditions.

Appendix F: Survey Implementation

The survey was implemented in Copiapó during June 2015, three months after the disaster. The affected and unaffected areas were defined through conversations with the local police, firefighters, and citizens. It was confirmed by official government images, a map marked by the local fire department after the flood, and satellite images. Half of the questionnaires were implemented in the exposed areas, and the other half in the unexposed areas.

Regarding the conjoint experiment, the candidates profiles were generated in advance to the implementation using *R*. Each questionnaire had eight pair of candidates attached at the end. The survey and conjoint were implemented in chapter.

The sampling strategy was exactly the same across the more and less affected areas. This is a key part of the design, because the differences between both sectors cannot be explained by differences in the implementation of the survey. The streets were selected following a random walk. On a given street, all households were invited to participate in the survey. By the end of the survey, almost all the town was accounted for. Only one sector was not included in the design, since it was partially affected and it is a relatively new area, so it could bring unwanted heterogeneity.

Appendix G: Local vs. National Elections

I focus on local elections because they were to be held in 2016, closer to the survey time, while national elections would not be held until 2017. In other words, it will not be realistic to make voters think about elections that will happen in two years.

It is important to evaluate whether we expect different results based on the type of leader selected (e.g., mayors vs. presidents). Are voters evaluating politicians at the local or national level?

In the case of Chile, social programs originate in the national government, but mayors play an active role in the implementation of these programs. For instance, even though a mayor cannot directly provide new housing, he or she plays a crucial role in asking the national government for more resources and coordinating their delivery. This is illustrated in the following picture, from a local newspaper in Paipote, which shows how the mayor, Maglio Cicardini (fourth from the left), participates in the ceremony transferring a house delivered by the national government to victims of the flood. Consequently, because of this complex relationship between the local and national governments, citizens have a hard time identifying who is actually providing these benefits.

Figure 1.8: Distribution of emergency houses (Source: Norte Noticias Diario Digital)

The following interview quote provides further support to this idea. Pamela, a Paipote resident, was selected to receive emergency housing from the national government. When she did not receive the new house on time, she went to the municipality to demand for her new housing: "I was supposed to receive emergency housing, and I have not got it. I went to the municipal community center, and even went to the municipality to talk about it." This shows how, even though the resources are allocated by the national government, local mayors play a role in their distribution. Therefore, the results of the conjoint experiment should be the same regardless if it focuses on mayors or presidents.

Appendix H: Robustness Checks

I have conducted two different robustness checks to test the sensitivity of my results to using a different treatment and sample.

When using the original treatment, 1 refers to reporting material damage, and 0 otherwise. In this robustness check, I redefine the treatment to make 1 equal to living in the area affected by the flood and 0 to living in an unexposed area.

The second robustness check tests the original specification in a matched sample. I used the `designmatch` package (Zubizarreta and Kilcioglu, 2016) to select the largest matched sample that reduces the standardized differences of the placebo covariates to be lower than 0.05. The new matched sample has 188 subjects; therefore, the matching procedure pruned 12 respondents to achieve the balance constraints defined beforehand.

The next table reports the results of the two robustness checks. The first model uses the original sample but an alternative treatment (area), while the second model uses the original treatment but an alternative sample (matched sample). I only report the δ coefficients (interactions) for left-wing candidates (in comparison to right-wing ones). The findings are consistent with the previous results: affected voters are rewarding candidates with a left-wing label.

Table 1.10: Robustness checks

	Outcome:	
	Electoral Choice	
	Area as Treatment	Matched Sample
	(1)	(2)
Left*Area	0.119*	
	(0.055)	
Left*Treatment		0.130*
		(0.057)
Respondents	200	188
Observations	3200	3008
Note:	*p<0.05; **p<0.01; ***p<0.001	

Appendix I: Regression Results

Table 1.11: Regression results

	Outcome
	Electoral Choice
Center	0.018
	(0.037)
Independent	0.005
	(0.033)
Left	−0.060
	(0.037)
Teacher	0.060
	(0.034)
Engineer	0.009
	(0.036)
Female	0.029
	(0.023)
40	−0.002
	(0.034)
50	−0.034
	(0.033)
Council member	0.006
	(0.032)
Mayor	0.046
	(0.036)
Will distribute a financial relief	0.260***
	(0.033)
Treatment	−0.047
	(0.066)
Treatment*Center	−0.007
	(0.054)
Treatment*Independent	0.116*
	(0.053)
Treatment*Left	0.124*
	(0.055)
Treatment*Teacher	−0.032
	(0.048)
Treatment*Engineer	−0.036
	(0.050)
Treatment*Female	−0.029
	(0.032)
Treatment*40	0.013
	(0.046)
Treatment*50	0.028
	(0.045)
Treatment*Council member	0.060
	(0.044)
Treatment*Mayor	0.004
	(0.047)
Treatment*Will distribute a financial relief	−0.002
	(0.046)
Constant	0.292***
	(0.047)
Note:	*p<0.05; **p<0.01; ***p<0.001

Appendix J: Alternative Hypotheses

Can traditional sanctioning arguments explain the previous findings? The blind retrospection theory argues that victims do not focus on candidates' ideology, but rather tend to punish incumbents as a way to channel their emotional distress. Consequently, candidates' ideological labels should not be relevant to voters when they make electoral decisions. However, the conjoint experiment shows exactly the opposite. Affected voters are more likely than non-affected voters to choose candidates with a particular ideological label.

The research design attempts to rule out the incumbent evaluation by focusing on hypothetical candidates who voters should not have a reason to punish or reward when making electoral choices. The conjoint experiment, however, can only partially discard the role of sanctioning arguments. For example, if affected citizens are rewarding the mayor and they associate him with the left, they would be more likely to vote for left-wing candidates.

Nevertheless, the evidence from the interviews does not support this alternative hypothesis. The mayor was the most blamed political actor: both affected and non-affected voters had a negative impression of his performance. The responses to the following survey question confirm the qualitative evidence: "Speaking about the floods, how would you rate the job performance of Mayor Maglio Cicardini in handling the disaster? (1) very good, (2) good, (3) neither good nor bad (fair), (4) bad, (5) very bad." The average response was 3.97.

Another option is that the mayor is associated with the right; therefore because he is being punished, victims are more likely to vote for the left. However, the mayor does not hold a clear ideological position. He was a member of the Socialist party (center-left) before running as mayor, but in 2008 he switched to the PRI (center) and in 2012 and 2016 ran as an independent (without party affiliation). Therefore, it does not seem that rewarding left-wing candidates is an alternative way to punish the incumbent mayor.

A different causal mechanism for explaining why affected and unaffected citizens have the same preference regarding distribution of financial relief is that the latter are expecting to also get a benefit even though they were not materially affected by the flood. The survey shows that only 5 % of non-affected respondents got aid from the state. Therefore, there

should be no reason to think that non-victims can expect to get financial aid if they were not exposed to the disaster.

Are disaster victims changing their preferences or they beliefs about left-wing policies and candidates? The table reports the impact of the treatment on self-placement on the ideological scale (from 1 (left) to 10 (right)) and a binary indicator of self-placement. There no significant distinction between both groups. Therefore, affected citizens are modifying their political preferences (i.e., stronger focus on welfare policies, in particular distribution of new housing) but are not changing their political beliefs (i.e., self-placement on the ideological spectrum).

Table 1.12: Regression results for respondents' ideology

	Outcome:	
	Ideological Position	Ideology Reported
	(1)	(2)
Treatment	−0.242	0.066
	(0.557)	(0.071)
Controls	Yes	Yes
Observations	88	200
Note:	*p<0.05; **p<0.01; ***p<0.001	

Are retrospective evaluations of authorities different across exposed and unexposed areas? Can those differences drive the main results? In the next table I show that the performance evaluations of the mayor and the president handling the disaster are statistically indistinguishable between both groups. Therefore, affected voters are not more likely to vote for a left-wing candidate because they have a worse or better opinion of the mayor or the president. The dependent variable has the following values to measures the performance of political authorities: (1) very good, (2) good, (3) neither good nor bad, (fair) (4) bad, (5) very bad.

Table 1.13: Regression results for authorities' evaluations

	Performance evaluation:	
	Mayor	President
	(1)	(2)
Treatment	−0.156	−0.101
	(0.140)	(0.143)
Controls	Yes	Yes
Observations	195	194
Note:	*p<0.05; **p<0.01; ***p<0.001	

Finally, I conduct diagnostic checks for the conjoint analysis following the Hainmueller, Hopkins, and Yamamoto, 2014 recommendations. I check the randomization of attributes by regressing respondent characteristics on the candidates' attributes. Additionally, I check that the results are not conditional to candidate order, which can have two dimensions: the order within a pair and the order across the eight pairs. I regress the outcome on the attributes, indicators of the order (candidate or pair), and the interaction between them.

Table 1.14: Balance test

	Outcome
	Respondent's gender
Center	−0.018
	(0.024)
Independent	0.018
	(0.022)
Left	−0.002
	(0.024)
Teacher	−0.012
	(0.021)
Engineer	−0.011
	(0.020)
Female	0.001
	(0.017)
40	−0.0005
	(0.018)
50	−0.006
	(0.015)
Council member	−0.015
	(0.019)
Mayor	−0.008
	(0.019)
Will distribute a financial relief	−0.015
	(0.017)
Constant	1.765***
	(0.039)

Note: *p<0.05; **p<0.01; ***p<0.001

Table 1.15: Profile order effects

	Outcome
	Electoral Choice
Center*Candidate 2	−0.030
	(0.051)
Independent*Candidate 2	−0.047
	(0.051)
Left*Candidate 2	−0.075
	(0.049)
Teacher*Candidate 2	0.057
	(0.044)
Engineer*Candidate 2	0.028
	(0.042)
Female*Candidate 2	−0.045
	(0.034)
40*Candidate 2	0.029
	(0.039)
50*Candidate 2	0.042
	(0.044)
Council member*Candidate 2	0.044
	(0.042)
Mayor*Candidate 2	0.065
	(0.039)
Will distribute a financial relief*Candidate 2	−0.004
	(0.036)

Note: Only reporting interaction terms. $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

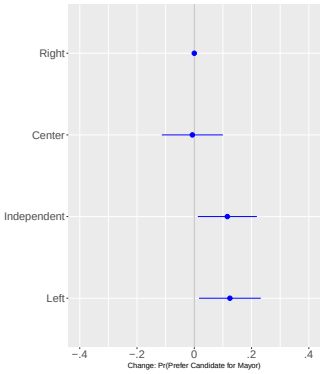
Table 1.16: Carryover effects

	Outcome
	Electoral Choice
Center*Pair 2	0.016
	(0.099)
Independent*Pair 2	−0.045
	(0.094)
Left*Pair 2	−0.036
	(0.094)
Center*Pair 3	0.077
	(0.097)
Independent*Pair 3	−0.122
	(0.090)
Left*Pair 3	0.054
	(0.098)
Center*Pair 4	0.126
	(0.098)
Independent*Pair 4	0.086
	(0.100)
Left*Pair 4	0.031
	(0.094)
Center*Pair 5	−0.061
	(0.100)
Independent*Pair 5	−0.063
	(0.087)
Left*Pair 5	−0.027
	(0.098)
Center*Pair 6	−0.004
	(0.097)
Independent*Pair 6	0.070
	(0.092)
Left*Pair 6	0.032
	(0.093)
Center*Pair 7	0.105
	(0.102)
Independent*Pair 7	0.082
	(0.094)
Left*Pair 7	0.152
	(0.100)
Center*Pair 8	−0.046
	(0.104)
Independent*Pair 8	0.041
	(0.093)
Left*Pair 8	−0.024
	(0.104)

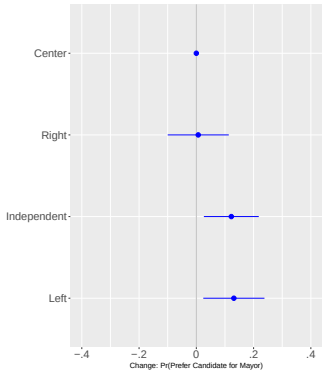
Note: Only reporting interaction terms of the main attribute of interest. $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

Appendix K: Other Reference Categories

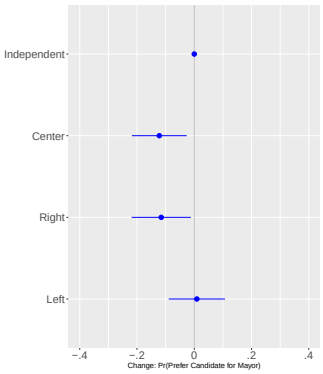
The main results were based on using a right-wing candidate as the reference category, but it is also possible to observe voters' preferences using the different ideological positions of the candidates as the baseline categories. The following figures report the results for the interactions (δ coefficients) but now also using independent, center, and left as reference categories. Only the results for the ideological attributes are reported.



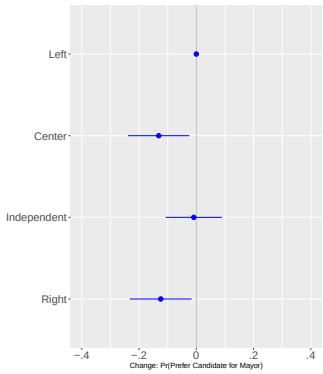
(a) Right-wing candidate as baseline category



(b) Centrist candidate as baseline category



(c) Independent candidate as baseline category



(d) Left-wing candidate as baseline category

Figure 1.9: Effects of the flood using different reference categories for ideology

Appendix L: Details about Behavioral Benchmark

The covariates used to select the control units were the following: right-wing parties[35] vote share in the 2012 local elections (Renovación Nacional, and Unión Demócrata Independiente); centrist parties vote share in the 2012 local elections (Partido Regionalista de los Independientes, ChilePrimero, and Fuerza del Norte); left-wing parties vote share in the 2012 local elections (Partido Igualdad, Partido Ecologista Verde, Partido Ecologista Verde del Norte, Partido Progresista, Partido Comunista, Izquierda Cristiana, Partido por la Democracia, Partido Radical Socialdemócrata, Partido Demócrata Cristiano, Partido Socialista, Movimiento Amplio Social, and Partido Humanista); independent candidates vote share in the 2012 local elections; human development index computed by the PNUD in 2003; poverty levels generated by the Ministry of Social Development in 2009, and demographic characteristics obtained from the 2002 national census.[36]

The following are the outcome variables: right-wing parties vote share in the 2016 local election (Renovación Nacional, Evolución Política, Partido Regionalista Independiente, and Unión Demócrata Independiente); centrists parties vote share in the 2016 local elections[37] (Partido Regionalista de Magallanes, Amplitud, and Somos Aysén); left-wing parties vote share in the 2016 local election (Partido Ecologista Verde, Poder, Partido Demócrata Cristiano, Partido Socialista, Partido Radical Socialdemócrata, MAS Región, Izquierda Ciudadana, Partido por la Democracia, Partido Comunista, Revolución Democrática, Partido Igualdad, Frente Popular, Fuerza Regional Norte Verde, Partido Progresista, Democracia Regional Patagónica, Frente Regional y Popular, Wallmapuwen, Partido Liberal, Partido Humanista, Movimiento Independiente Regionalista Agrario y Social, and Unión Patriótica); independent candidates vote share in the 2016 local elections; age

[35]Center-left wing parties are considered as left-wing, meanwhile center-right are considered as right-wing.

[36]The following are the exposed counties: Antofagasta, Taltal, Copiapó, Caldera, Tierra Amarilla, Chañaral, Diego de Almagro, Vallenar, Alto del Carmen, Freirina, and Huasco. Meanwhile, the following are the selected control counties that meet the covariate balance requirements: Calama, Quilpue, Maria Elena, Calera, Illapel, San Pedro de Atacama, Quintero, Panquehue, Combarbala, Algarrobo, and San Antonio.

[37]Some of the parties that were considered in the center of the ideological spectrum in 2012 now are right-wing parties because they joined the list of the center-right coalition.

of candidates; and education of candidates.[38]

The goal of the mathematical algorithm used in the chapter was to generate the largest matched sample that is balanced in terms of observed covariates. The balance requirements can be defined beforehand by the researchers. In this case, I focus on mean balance. This means that standardized differences between both groups should be below a particular threshold. I use the `designmatch` package in `R` (Zubizarreta and Kilcioglu, 2016) and the `Gurobi` optimizer to obtain the control group.

[38]Binary indicator of more than high school constructed using the public declaration of patrimony.

Appendix M: Interviews in Spanish

Carmen: "Cuando ocurrío el aluvión yo estaba acá en mi casa, y a las 4 de la mañana llegan los bomberos con la sirena informando que teníamos que evacuar toda esta calle, porque se podría ver toda esta parte inundada. Y de ahí nosotros evacuamos hacia cerca de la plaza, un poco más allá, y como a las 6 de la mañana ya empezó a llover más fuerte. Y después como a las 12, ya en la casa de mis abuelos, se vino toda el agua encima, ya no pudimos arrancar ni nada. **Investigador**: Donde queda la casa de sus abuelos, por que parte? **Carmen**: Por 21 de mayo con Juan López, por la avenida principal. **Investigador**: Y esa parte fue afectada, por las ...? **Carmen**: Si, toda esa parte fue afectada. Nosotros evacuamos mejor allá para tener más resguardo, porque yo tengo un bebe. **Investigador**: Usted esperaba que esta parte iba ser más afectada que allá abajo, pero fue al revés al final? **Carmen**: Claro, porque aca no paso nada. Como a las 12 del día se empezo a salir toda el agua de la defensa, y ya no teníamos nada que hacer. Tuvimos que empezar a subir algunas cosas de nuestros abuelos, salvarles ropa, y la comida, porque sabíamos que se venían varios días sin luz sin agua. Y tuvimos que arrancar por el patio, alcanzamos a rescatar un escalera y un primo la puso y tuvimos que salir arrancarnos todos por los patios. Ahí nos resguardamos en una casa de una vecina de mis abuelos, pero igual el agua hasta por aca, mi bebe quedo flotando en la cama. Entonces fue como super angustiante. Y en un momento yo igual pense que me iba a morir, era tanta la fuerza del agua, y con tanta rapidez que llego y entro que nosotros no sabiamos que hacer. Y habíamos muchos más ahí porque todos nos fuimos para allá. **Investigador**: Usted vive acá, esta es su casa? Y aqui que fue lo que sucedió? **Carmen**: Aca la lluvia no más, igual vino un poco de barro, porque más arriba igual se salio un poco de la defensa y todo. Pero justo esta parte como que no fue mucho, solamente barro pero que no alcanzo a entrar a las casas."

Daniela: (1) "Todos los proyectos que uno tenía tuvieron que cambiar, tuvieron que retroceder. Muchos se cortaron y cambiarlos por otros. El arreglo de la casa, porque ayuda no hemos tenido. (...) La prioridad en este momento es la casa, lo otro paso a segundo plano." (2) "Dar solución a los problemas y no poner parches, es lo principal (...). Basarse

en cosas más importantes o darle prioridad a las cosas que realmente tiene relevancia (...) Es más principal arreglar una casa donde un niño necesita su hogar para vivir que un paradero donde parar una locomoción colectiva."

Pedro: "No es tanto económico sino yo diría una ayuda más física, más como dice mi hermano, un cierre, unas casas, una vivienda definitiva. En construcciones, porque en lo económico no sirve (...) Así que la mejor ayuda sería en forma física, cierres, casas. O sea no estamos pidiendo la media casa tampoco, sino algo así como para que nos construyan algo para nosotros seguir construyendo de frente, una idea fija."

Rosa: "Luego de las inundaciones cambia todo. (...) Yo tenía aspiraciones, tenía sueños, y eso quedo ahí (...) Para mi ha sido complicado, mi hijo tuvo que dejar la Universidad, para mi ha sido fuerte también. (...) No pense que después del 25 de marzo me iba a cambiar tanto la vida. No pense. Tampoco creía que me podía suceder una cosa así. Despues del 25 de marzo yo pense que era un sueño. El 26 de marzo yo veía mi casa llena de barro, y decía chuta, va a llegar la noche y donde voy a dormir. Mañana voy a despertar y estoy no va a estar, porque esto es un sueño."

Manuel: "Yo creo que cuando uno elige a alguien no es por esas situaciones, o cosas puntuales o especificas (beneficios), así yo le voy a dar esto. No po, tiene que ser un compromiso más general con la comunidad (...) El bono tanto para este, son dos lucas, tres lucas, y a quien le sirve eso, en el tiempo a nadie. Tiene que ser algo concreto, porque yo le puedo decir le voy a dar este beneficio pero la gente va seguir en lo mismo. No son soluciones definitivas, son de momento".

Claudia: "A mi me gustaría que el alcalde que saliera o fuese electo se preocupara de la calidad de vida de las personas (...). Pero si, que se preocupe de la calidad de vida en todo aspecto, en todo aspecto, no que me entreguen una bolsa con comida, no, otras cosas, también."

Ana: "Y después escuchar los testimonios de la gente, escuchar que pudieron salvar sus vidas, amarrarse a las rejas para que el agua no los llevara (...) niñitos que perdieron todo."

Tania: "Yo una vez me acuerdo que me vine en la micro y habia una pareja de abuelitos que habian ido a comprar, y la micro los dejo y yo me ofrecí a encaminarlos, y la abuelita me dijo sabe que hija perdimos todo, la casa, me dijo la casa era mia dijo, y yo tenía de allegada a mi hija, a mi hija le dieron y a nosotros no (...) Como veis tu, si se supone que la casa era de la abuelita, y eran dos familias, deberian darles a los dos por iguales y le dieron a una."

Pamela: "Me tenían que dar la vivienda de emergencia, y hasta ahora no me la han dado. Yo fui aquí hay un centro comunitario de la muni, fui y conversé, incluso fui con el a conversar a la alcaldía."

Chapter 2

Self-Interested Citizens: How Disaster Victims Modify their Political Priorities

Abstract

Do disaster victims care only about their personal well-being or do they also focus on collective concerns? Specifically, are they more likely to prioritize housing (i.e., individual well-being), infrastructure (i.e., social well-being), or both? I answer this question by using survey data before and after the sixth largest earthquake ever documented, and by combining a difference-in-differences strategy with matching to overcome some methodological challenges. I find that affected citizens prefer to stress the importance of individual gains but are not more likely to emphasize the distribution of collective goods. This reveals that disaster victims are self-interested and myopic: they focus exclusively on their immediate material concerns and are not able to see that they can also improve their living conditions by benefiting from public works. These findings have important political implications for learning about citizens' selfishness and altruism and for better understanding the causal mechanisms behind disaster victims' electoral decisions.

2.1 Introduction

In seeking to understand the political consequences of natural disasters, the extant literature tends to focus on how these negative events affect the incumbent vote share. The traditional explanation for affected citizens punishing the incumbent relies on a process of (mis)attribution of responsibilities. For example, disaster victims might be myopic and always blame the incumbent (Achen and Bartels, 2016), or might only punish the current leader after their poor performance handling the disaster (Gasper and Reeves, 2011; Healy and Malhotra, 2010).[1]

These explanations, however, neglect to take into account how, before influencing victims' electoral decisions, catastrophes change these individuals' preferences and attitudes: for example, modifying their democratic values (Carlin, Love, and Zechmeister, 2014) and attitudes toward civic engagement (Fair et al., 2013). Thus, in addition to the evaluation of incumbent performance, other mechanisms might also be relevant for understanding how affected citizens make electoral choices.

One such mechanism, overlooked by previous studies, is the role played by disaster victims' new priorities and concerns. These might include, for instance, the reconstruction of their houses and the repair of public infrastructure. According to the issue ownership theory of voting (Bélanger and Meguid, 2008; Petrocik, 1996), victims' electoral decisions can be influenced by the salience of particular issues: in the case of disaster victims, improving their standard of living. Therefore, affected citizens should be more likely to prefer candidates who can better address these new political priorities.

How do citizens change their priorities after a catastrophe? Do victims tend to focus more on collective or individual concerns? Though we would expect disaster victims' priorities to shift, we do not know what their top priority will be. There are two likely possibilities. First, we can imagine that disaster victims may become more likely to prioritize collective concerns, such as the improvement of community infrastructure like schools, hospitals, and railroads. A second alternative is that they will prioritize their personal wel-

[1] Victims might also be incorporating new information about the incumbent (Ashworth, Mesquita, and Friedenberg, 2014).

fare, by favoring initiatives like the distribution of financial relief and the construction of new housing.

To explore disaster victims' new priorities, I focus on the sixth largest earthquake ever documented. On February 2010, Chile was shattered by a massive earthquake affecting six out of the fifteen regions of the country. Chile provides a unique opportunity to test the political consequences of earthquakes: because all its regions have been affected by this type of catastrophe in the past, therefore, there is greater comparability between counties that were and that were not exposed to this particular disaster.[2] In addition, earthquakes are exogenous shocks, so they cannot be predicted or anticipated.

To study how the earthquake modified victims' priorities, I rely on survey data before and after the negative event comparing areas exposed and not exposed to the disaster. When using survey data, however, sampling variability and the use of particular geographic areas as treated and control groups can create imbalances within each group across time. This lack of balance can translate into bias when estimating a difference-in-differences (DID). It is important to remember that it is not problematic if the composition of the treated and control group is different. Results can be biased, however, if one of the two groups is different across time since it will not be possible to know if the estimate is a result of the treatment or a consequence of a different group composition. Later I show how there are significant differences across time in one of the two groups regarding their observed characteristics.

In order to reduce imbalances across time, I rely on advances in optimal matching and mathematical programming[3] to construct a synthetic panel using three waves of surveys. Two were conducted three and six months before the earthquake, and one was implemented three months after it. The synthetic panel aims to generate comparable groups of people before and after the earthquake, such that sample composition is similar across periods. I compute the intensity of the earthquake at the county level to identify exposed and control respondents.

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[2]The 2010 earthquake mainly affected the south-central region of the country, so neither the south nor the north were exposed.

[3]See Zubizarreta and Kilcioglu, 2016.

I use a differences-in-differences (DID) approach in the synthetic panel to study how exposure to the 2010 earthquake affected victims' political priorities. Additionally, I implement recommendations from the statistical theory of design sensitivity (Rosenbaum, 2004), which shows that research design can help reduce sensitivity to hidden biases. In particular, I focus on the use of extreme exposures to the treatment to achieve this goal (Rosenbaum, 2011; Zubizarreta, Cerdá, and Rosenbaum, 2013).

The outcome of interest are respondents' priorities. To gauge these, the survey asked respondents to select the country's three most significant problems from a pool of alternatives. After a disaster that caused massive damage to not only houses but also bridges, roads, ports, and airports (Hinrichs, Jones, and Stanley, 2011), victims might be expected to be more likely to identify both housing and infrastructure as two of their three main priorities. This hypothesis only partly bears out. The findings show that exposure to the earthquake increased by 22 percentage points the likelihood of reporting housing as one of the most critical problems to be addressed by the government. Exposure to this natural disaster, however, did not increase concerns about infrastructure. Since the total cost of the disaster was estimated to be US 30 billion, or 18% of the Chilean Gross National Product (McClean, 2012), these results are surprising: despite the devastating consequences of the catastrophe, affected citizens were not more likely to prioritize infrastructure. These findings reveal to us that disaster victims have myopic interests since they prioritize the reconstruction of their homes but not the improvement of their communities.

This chapter provides three main contributions to the literature about the political consequences of natural disasters. First, it focuses on an unexplored political effect of catastrophes: their impact on citizens' priorities and concerns. The extant literature mostly pays attention to victims' electoral choices, traditionally measured in terms of the incumbent vote share. These choices, however, might not only be explained by victims' evaluation of an incumbent's performance, but also by their new preferences, attitudes, and priorities, the focus of this chapter. Second, the survey results discussed here allow us to better understand how citizens react to negative events: specifically, if they have individual or pro-social attitudes. Because individuals in the developing world are constantly exposed to situations that diminish their living conditions, it is important to further illuminate how they

update their political preferences in different scenarios. Finally, the chapter contributes to the ongoing conversation about the design of observational studies, by using strategies that reduce some of the problems associated with using survey data for DID approaches.

2.2 The Political Consequences of Natural Disasters

There is a growing body of literature studying the political consequences of natural disasters. Most of this research focuses on two outcomes: incumbents' vote share (Achen and Bartels, 2004; Bechtel and Hainmueller, 2011; Cole, Healy, and Werker, 2012; Gasper and Reeves, 2011; Healy and Malhotra, 2010; Lazarev et al., 2014; Remmer, 2014) or turnout (Chen, 2013; Gomez, Hansford, and Krause, 2007; Lasala-Blanco, Shapiro, and Rivera-Burgos, 2017; Sinclair, Hall, and Alvarez, 2011).[4]

Another area of research, however, focuses on how affected citizens change their preferences and attitudes after natural disasters. In one such study, by Carlin, Love, and Zechmeister, 2014, of the impact of the 2010 Chilean Earthquake on democratic legitimacy, the authors find that disaster victims are less supportive of their local governments, less politically tolerant, and more supportive of military coups. In another study, Fair et al., 2013 show that the 2010 floods in Pakistan changed citizens' attitudes and civic engagement: in particular, that the disaster increased aggressive civic engagement. Studying the same floods, Kosec and Mo, 2015 provide evidence that this disaster also decreased affected citizens' aspirations, especially among the poor. Finally, Healy and Malhotra, 2009 use data on natural disasters, government spending, and electoral results in the US to show that voters reward the incumbent presidential party for delivering disaster relief but not for investing resources in preparedness.

In an attempt to better understand how natural disasters affect victims' political preferences, we must ask whether these events change their priorities, and if so, what their new concerns are. As we know, political priorities correspond to particular issues that are salient for citizens. For instance, surveys commonly ask respondents to define the most significant

[4]There is also a body of research that explores the process through which responsibility is attributed (Arceneaux and Stein, 2006; Atkeson and Maestas, 2012; Gomez and Wilson, 2008; Maestas et al., 2008; Malhotra and Kuo, 2008).

problem in their country. Individuals who mention crime, for example, are prioritizing this issue over others such as education and health.

The issue ownership theory of voting argues that citizens identify the party or candidate that can address the most salient issue they care about when making electoral choices. In consequence, voters should be more likely to prefer parties or candidates that fit with their main concerns (Bélanger and Meguid, 2008; Petrocik, 1996). Nevertheless, priorities are not exogenous variables, since they can be affected by long-term traits that can also determine voting decisions. For instance, right-wing voters are more likely than left-wing ones to identify crime as a crucial priority (Mayer and Tiberj, 2004). Within the context of this theory, an earthquake provides an opportunity to study how an exogenous shock can affect voters' concerns.

In this chapter, I focus on two different types of priorities: individual and collective. The first refers to issues that mainly provide personal gains. These include the distribution of private goods such as food baskets, financial relief, and new housing after natural disasters, since they deliver gains to victims on an individual level.[5] The second group of priorities refers to issues of a collective nature that benefit groups of people. These include the distribution of public goods such as the repair of railroads, schools, and hospitals after natural disasters. This chapter aims to answer to the following question: are affected citizens more likely to focus on individual, collective, or both types of concerns after a natural disaster?

I theorize that the combination of these priorities may generate four main types of victims. First, *unresponsive victims* will be more likely to not prioritize housing (i.e., individual concern) or infrastructure (i.e., collective concern) after a shock. Second, *self-interested victims* will only prioritize personal gains such as housing, and not the construction of public works such as infrastructure. Third, *pro-social* victims will only pay attention to collective benefits and not to personal concerns. Fourth, *attentive victims* will be more likely to focus on both dimensions of post-disaster welfare: the distribution of both private and public goods. By using an empirical strategy based on comparing victims and non-victims before and after the earthquake, it is possible to identify which of these four categories best

[5]New housing can improve the quality of the neighborhood as a whole, but it is still an individual benefit.

describes disaster victims' political preferences.

The findings of this study have meaningful implications for understanding how disaster victims make electoral decisions. For example, if they only pay attention to private benefits, they may be more likely to vote for politicians associated with the distribution of short-term handouts (e.g., clientelistic candidates) and/or with the implementation of social programs such as new housing (e.g., left-wing candidates). Conversely, if they focus on public benefits such as the repair of infrastructure, they may be more likely to pay attention to valence issues that provide information about the competence of candidates to manage and address the consequences of a catastrophe.

It is natural to ask whether post-disaster priorities are a valence or a policy concern, and I hold that they are both: housing can have a programmatic dimension since it represents a particular kind of social or welfare policy. At the same time, infrastructure can have a non-programmatic component because the reconstruction of public works can be connected with leaders' managerial capacity to handle the post-disaster scenario.

The survey results provide evidence supporting the idea that disaster victims are self-interested, according to the definition laid out above, and only focus on private benefits. Even though the earthquake has severely damaged their communities, they are not more likely to prioritize the reconstruction of public infrastructure. To understand these results, it is important to distinguish between rationality and selfishness (Edlin, Gelman, and Kaplan, 2007). Voters can be rational by not prioritizing collective goods that can also improve their living conditions, because they are only focused on the reconstruction of their houses, and unable to see the problem from other angles. Simply put, disaster victims are rational but myopic.

My analysis also addresses a possible concern regarding variation in material damage experienced within the exposed group. In the affected regions some people lost their houses and essential belongings but others living in the same area did not. Because of this variation, the emphasis only on housing could be read as pro-social rather than self-interested: less affected citizens may care about the reconstruction of houses, even though they will not benefit from this program. To address this possible concern I focus only on exposed counties that were severely affected by the earthquake. Specifically, a treated county is

the one where the strength of shaking produced by the catastrophe was above a particularly high threshold. This decision helps to better identify affected respondents and reduce sensitivity to hidden biases as explained in the following section.

Another way to interpret the main findings is by understanding citizens' decision-making process as based on a trade-off between private and public goods. Victims first focus on their own home and only then do they pay attention to infrastructure. In this sense, the results might not indicate self-interested or myopic behavior. Nevertheless, it is worth remembering that the outcome is constructed using a question where respondents select three priorities and not just one. So, if there were a trade-off between priorities occurring, we might observe disaster victims selecting housing first and then infrastructure second or third, which is not the case. Affected citizens are not more likely to mention infrastructure as a first, second, or third concern. As a point of comparison, affected respondents are four times more likely to prioritize judicial reforms and almost ten times more likely to focus on combating drugs than on infrastructure. It is surprising that this issue is not a concern after a devastating earthquake with so great a magnitude that the day was shortened by 1.26 microseconds (Buis, 2010).

2.3 The 2010 Earthquake in Chile

The 8.8 earthquake that shook the central-southern regions of Chile in February 2010 was, according to the United States Geological Survey, the sixth largest ever documented.[6] More than 12,000 people were injured and more than 500 were killed by the catastrophe (Choi, 2012). Across Chile, six out of fifteen regions were officially declared affected areas by the government.

The disaster devastated the exposed cities and localities. Based on the official reconstruction plan, 220,000 houses suffered severe damage or were destroyed, 4,353 schools were damaged, 40 hospitals were severely damaged, and 17 hospitals were completed destroyed. In terms of public infrastructure, the country had 1,554 kilometers of damaged roads, 212 bridges destroyed or almost destroyed, and nine airports that suffered different

[6]United States Geological Surveys, "20 Largest Earthquakes in the World."

degrees of damage (Government of Chile, 2010). With such largescale destruction, we would expect disaster victims to be increasingly concerned about housing and infrastructure. The postdisaster reconstruction indeed became the most pressing challenge for the president of the country Sebastian Piñera. He proposed a reconstruction plan of US 8.431 billion distributed to the ministries of housing, education, health, and public works (Arana Araya, 2016). The state's response to this disaster can be divided into two stages: In the first, the state provided an immediate response, which took the form of emergency aid. In the second, it focused on reconstruction and rebuilding (Sehnbruch et al., 2016).

This disaster had long-term consequences in the affected regions. As the United Nations Office for Disaster Risk Reduction reports, the repercussions from the earthquake were still an issue more than two years after the shock (McClean, 2012). In 2013, the year of the subsequent presidential election and three years after the earthquake, the government was still delivering new houses in the affected counties.[7] In summary, this disaster was strong enough to modify affected citizens' priorities toward the reconstruction and repair of damaged houses and public infrastructure.

2.4 Research Design

To study disaster victims' political priorities, I exploit three nationally representative surveys, two implemented three and six months before the earthquake and one conducted three months after it. These surveys were implemented by the same institution and followed the same sampling strategy.[8]

I implement a difference-in-differences strategy (DID) with these three surveys. The assumption underlying this empirical strategy is that the treatment and control outcomes move in parallel trends when there is no treatment, such that any divergence from these paths can be interpreted as a treatment effect (Angrist and Pischke, 2014). According to this logic, it is possible to identify two groups in the post-disaster survey, individuals living in exposed and non-exposed areas, as well as two groups in the pre-disaster surveys,

[7] La Nación, "Entregan 150 viviendas en Yumbel para damnificados del terremoto."

[8] Surveys from the Centro de Estudios Públicos (CEP). All use a probabilistic sampling strategy.

respondents living in areas that will and will not be exposed to the earthquake. Though we might think that residents of exposed and unexposed areas are different across multiple unobserved covariates, by including pre-disaster surveys, we only need to assume that there are parallel trends within these groups across time.

Obviously, DID presents some limitations. First, when the treatment is as good as random this is an appropriate empirical strategy for avoiding common endogeneity issues (Bertrand, Duflo, and Mullainathan, 2004). The treatment, in this case, is exposure to the earthquake. Because this type of natural disaster cannot be anticipated and the entirety of Chile has been exposed to earthquakes in the past, all counties are eligible for treatment. This feature increases the comparability between counties from different regions.

Second, when implementing a DID with survey data and focusing on particular regions of a country, the composition of one of the groups pre/post-intervention might not be stable across surveys due to sampling variability. This issue might threaten the validity of the DID, because groups might no longer be comparable across time. The lack of comparability, or in other words, the existence of imbalances in one of the groups, can lead to biased results since we will not know if a treatment effect is explained by the intervention or by the different group composition across time.

To address this potential issue, I use matching to construct a synthetic panel that guarantees covariate balance between the pre/post control and exposed groups, and implement a DID strategy in this matched sample.[9] To achieve covariate balance, and consequently to construct the synthetic panel, I use the designmatch package (Zubizarreta and Kilcioglu, 2016). This provides a flexible matching approach that allows us to obtain different forms of covariate balance (Resa and Zubizarreta, 2016). In this case, I use fine balance, which focuses on balancing the marginal distributions of the exposed and control groups exactly in aggregate but does not constrain who is paired with whom as exact matching does (Rosenbaum, Ross, and Silber, 2007). Put simply, if in the exposed group there are five women and ten men, after using fine balance, in the matched control group there will be five women

[9] O'Neill et al., 2016 propose adjusting for past outcomes before implementing the DID. However, that approach requires actual panel data.

and ten men, but a woman does not have to be paired to a woman.[10] I use fine balance because, as oppose to mean balance, it guarantees covariate balance across multiple waves.[11] Furthermore, it is less restrictive than exact matching since it does not focus on pairing.

The process of constructing the synthetic panel has seven steps. First, I define the covariate balance requirements. There are not any pretreatment covariates available because I use survey and not panel data. In this case, it is best to use individual characteristics that will not be affected by the treatment (Gelman and Hill, 2007; Rosenbaum, 1984). I adjust for age,[12] education,[13] gender,[14] and voter registration.[15] Second, I use matching in the post-disaster survey to find a matched sample that satisfies the covariate balance requirements (i.e., fine balance). Third, I use the matched exposed group (from step 2) as a baseline to construct the synthetic panel.[16] I match that group with the control group from the first survey implemented before the earthquake (i.e., respondents living in an area that would not be exposed to the disaster). Fourth, I use the baseline group again but now match it to the exposed group from the first pre-disaster survey (i.e., respondents living in the area that would be exposed to the disaster). Fifth, I again use the baseline group, matching it to the control group from the second survey implemented before the earthquake. Sixth, I return to the baseline group and match it to the exposed group from the second pre-disaster survey. Finally, after the first six steps, I have multiple groups with the same composition of age, gender, education, and voter registration (due to the fine balance constraint). I merge

[10]This assumes that there has been no pruning of observations in the treated group to achieve covariate balance.

[11]For example, if the mean balance constraint is a standardized difference of 0.1 between a group in wave 1 and wave 2, it will be possible to see a difference of 0.2 between wave 1 and wave 3.

[12]1: Less than or equal to 29 years old, 2: 30-39 years old, 3: 40-49 years old, 4: 50-59 years old, 5: 60-69 years old, 6: greater than or equal to 70 years old.

[13]1: no education or primary education incomplete, 2: primary education complete or secondary education incomplete, 3: secondary education complete, 4: higher education no college, 5: higher education college.

[14]1: female, 0: male.

[15]In 2010, only registered citizens were allowed to vote. Registration was voluntary and voting mandatory for registered citizens. Therefore, this is a good proxy of interest in politics. Registration should not be affected by the earthquake because it happened in February 2010 and the next election was not until October 2012. Affected citizens did not have an incentive to register to vote three months after the disaster if the next elections were 28 months away.

[16]Using the matched exposed or control group as the baseline would be the same.

them to form the synthetic panel of survey data.

Algorithm 1 Construction of synthetic panel

1. Specify the covariate balance requirements (fine balance for placebo covariates).
2. Find a matched sample that satisfies the covariate balance requirements for the post-disaster sample.
3. Use the matched exposed group (step 2) as a reference to match it with the control group from the first pre-disaster sample.
4. Use the matched exposed group (step 2) as a reference to match it with the exposed group from the first pre-disaster sample.
5. Use the matched exposed group (step 2) as a reference to match it with the control group from the second pre-disaster sample.
6. Use the matched exposed group (step 2) as a reference to match it with the exposed group from the second pre-disaster sample.
7. Merge all the matched samples.

The synthetic panel is based on identifying a pre/post exposed and control group before conducting the matching to achieve covariate balance. To identify exposed units, I use the peak ground acceleration at the county level. This indicator measures the strength or intensity of shaking produced by the earthquake in a given geographic area. This is determined from effects on people, human structures, and the natural environment.[17] Unlike the traditional Richter scale, this metric does not capture the energy released but "how hard the earth shakes in a given geographic area" (Bhushan, 2011). The exposed counties are the ones with a peak ground acceleration (PGA) greater than 0.275 g, which is a traditional cutoff to identify localities severely affected by an earthquake (Visconti and Zubizarreta, 2017; Zubizarreta, Cerdá, and Rosenbaum, 2013). The control counties correspond to all the places located in the non-affected regions based on the official reconstruction plan generated by the government (Government of Chile, 2010). Consequently, individuals living in the exposed counties are exposed units, and respondents living in the unexposed counties

[17] USGS: Magnitude/Intensity Comparison

are control units.[18]

Figure 2.1: Map of Chile. The regions that were declared affected by the government are in red. This a modified version of the map provided in the Reconstruction Plan (Ministry of Housing and Urban Development, 2010).

This strategy helps to exploit a dose-response relationship: by analyzing a subpopulation in which the treatment effect is larger we can better identify the association between

[18]The existence of spillovers could be a concern, where internal migration from exposed to unexposed counties could affect the results. However, the reconstruction plan attempted to avoid this situation. Its main goal was to "maintain neighborhood social networks, consolidate existing settlements, and avoid migration from rural areas" (Government of Chile, 2010). In addition, I expect any migration from affected to unaffected areas to bias the effect towards 0. Therefore, any positive effects should be a conservative estimate.

the treatment and the outcome (Rosenbaum, 2017). The use of extreme treatment conditions helps reduce sensitivity to hidden biases (Rosenbaum, 2004), while the inclusion of marginal exposures can make conclusions more sensitive to unmeasured biases (Zubizarreta, Cerdá, and Rosenbaum, 2013). Thus, based on the goal of comparing subpopulations experiencing very different exposures to the treatment, I focus on counties severely affected by the earthquake and counties not exposed to it.

To measure voter priorities, I use the following survey question: "Which are the three problems that the government should dedicate the greatest effort to solving?" Respondents need to enumerate three of these problems.[19] I construct two binary indicators if they mention *infrastructure and public transportation*[20] or *housing*[21] as one of three main issues the country is facing.[22] I use the following equation to estimate the effect of the 2010 earthquake on voter priorities. The units of observation are the survey respondents.

$$Y_i = \alpha + \beta_1 T_i + \beta_2 P_i + \beta_3 T * P_i + \beta_4 X_i + \sigma_n + \varepsilon_i \tag{2.1}$$

In this DID model, the key parameter of interest is the interaction term β_3, which captures the differences between groups and over time. Y is one of the two binary indicators for the outcome of interest (infrastructure or housing). T depicts the treatment (living in an area exposed to the earthquake or that will be exposed to the earthquake), P describes a post-disaster indicator (survey conducted after the earthquake). X corresponds to the set of covariates used to obtain balance. σ_n represents county fixed effects. I clustered standard errors at the treatment level.

The treatment is the intensity of the earthquake in a given county. The treatment, however, does not only capture damage from the disaster but also the government response to it. In other words, affected citizens both experienced the direct consequences of the earthquake as well as witnessed the government providing public and private goods to af-

[19]The survey included a battery of problems to be selected such as crime, drugs, electoral reform, human rights, corruption, etc.

[20]In the case of the earthquake, this concern can be linked to the reconstruction of public schools, hospitals, and railroads.

[21]In the case of the earthquake, this concern can be linked to the provision of emergency housing.

[22]The same question is asked three times, and respondents need to identify three problems. The binary indicator captures whether infrastructure or housing are included among these three main problems.

fected regions. The peak ground acceleration at the local level thus represents a compound treatment.

How might this affect the interpretation of the main results? Could the main findings be a consequence of the disaster response? Specifically, if the government was only dedicating resources to the reconstruction of public infrastructure, might this explain disaster victims being more likely to prioritize housing? This was not the case in reality: the government response after the earthquake focused on both individual and collective concerns, including both the reconstruction of critical public infrastructure such as hospitals and schools, and the provision of emergency housing (Government of Chile, 2010; Samaniego, 2010). As a result, disaster victims should not be more likely to identify housing as a consequence of the government strategy.[23]

2.5 Results

Table 2.1 depicts the challenges of using survey data when implementing a difference-in-differences strategy. It is important to remember that if the exposed but not the control group (or vice-versa) is different before and after the intervention in term of their observed covariates, the parallel trends assumption will be hard to hold. Table 2.1 shows the mean voter registration in the treated and control groups (before matching), which clearly shows an unstable pattern across time for only one of the groups (see more examples using age and education in the supplementary appendix).

Table 2.1: Mean voter registration (before matching)

Survey	Treated	Control
August 2009	0.833	0.728
October 2009	0.814	0.774
June/July 2010	0.746	0.740

Table 2.2 displays the results of a t-test comparing groups across surveys. The evidence shows clear imbalances in respondents' voter registration across the surveys but only in the treated group (i.e., p-values lower than 0.1).

[23]The earthquake happened one week before the beginning of the academic year, so the government paid special attention to the reconstruction of schools. Furthermore, the provision of housing was a high priority since winter was only four months away.

Table 2.2: P-value voter registration (before matching)

Surveys	Treated	Control
August 2009 - October 2009	0.565	0.194
August 2009 - June/July 2010	0.009	0.733
October 2009 - June/July 2010	0.046	0.323

One way to obtain covariate balance and, thus, achieve comparable samples is to use matching. Figure 2.2 shows how fine balance works, and in particular, how these three surveys are now comparable regarding observed covariates after matching. As a result, the parallel trend assumption becomes more credible.[24]

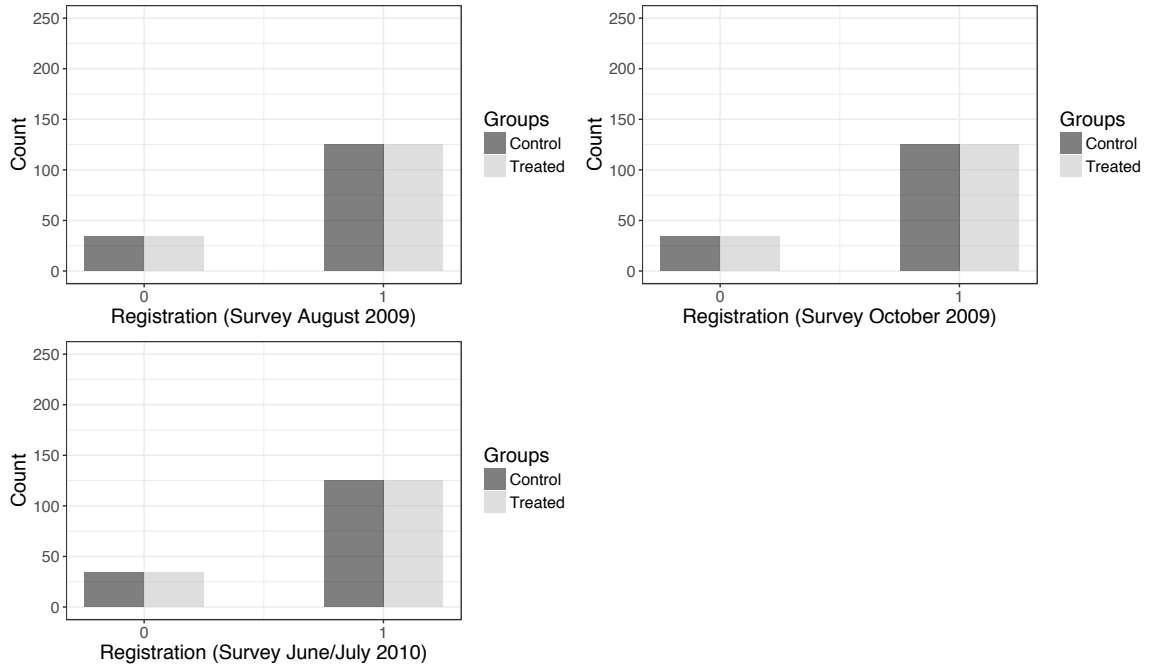


Figure 2.2: Fine balance after matching

The next two tables show the means and the p-values for the t-test after matching, which illustrate how these groups are now comparable across time.

Table 2.3: Mean voter registration (after matching)

Survey	Treated	Control
August 2009	0.786	0.786
October 2009	0.786	0.786
June/July 2010	0.786	0.786

[24]The matching algorithm maintains 55% of the available units in order to achieve covariate balance.

Table 2.4: P-value voter registration (after matching)

Surveys	Treated	Control
August 2009 - Survey October 2009	1.00	1.00
August 2009 - Survey January 2010	1.00	1.00
October 2009 - Survey June/July 2010	1.00	1.00

Figure 2.3 displays the evolution of outcomes across time using the matched sample to empirically observe voter priorities before and after the February 2010 earthquake.

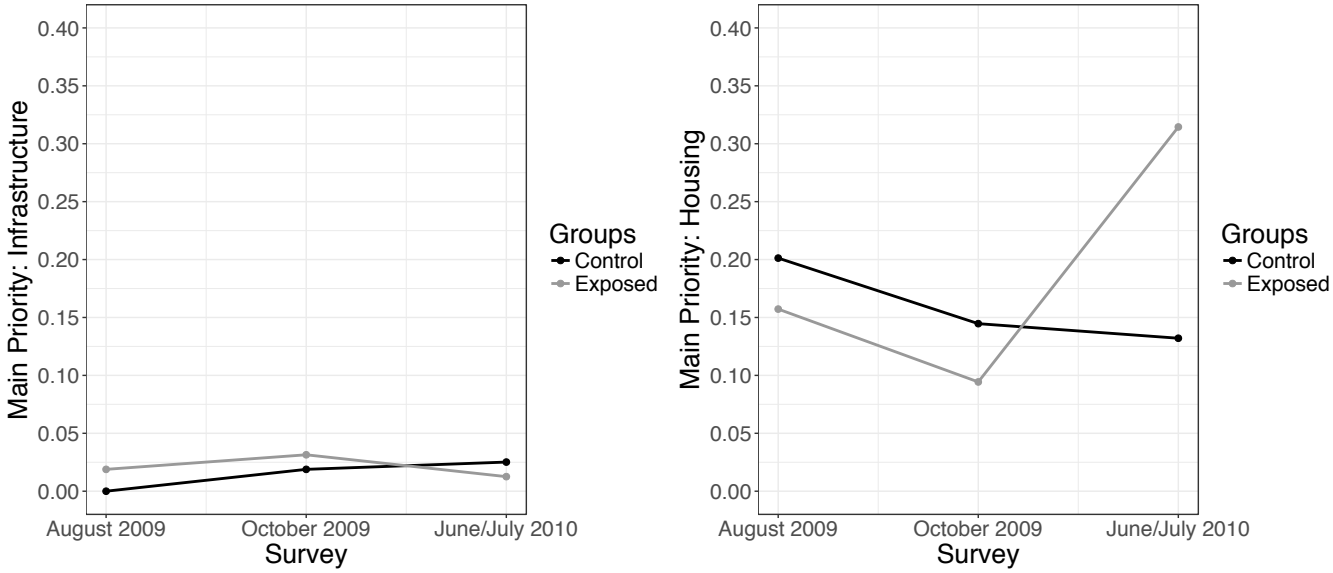


Figure 2.3: Priorities across time (matched sample)

This plot illustrates the stability of voter priorities about infrastructure regardless of the devastating consequences of the earthquake, and how exposed respondents dramatically modify their concerns about housing after the earthquake. Table 2.5 reports the β_3 coefficient (interaction term) when using equation 1 in a matched sample with the post-treatment survey (three months after the disaster) and two pre-treatment surveys (three and six months before the disaster).

Table 2.5: Regression results

	Priorities	
	Infrastructure	Housing
	(1)	(2)
Treatment*Post (β_3)	−0.008	0.219***
	(0.016)	(0.075)
Observations	954	954

Variables not shown: treatment, post, placebo covariates, and county fixed effects.
*p<0.1; **p<0.05; ***p<0.01

The key coefficient of interest in equation 1 corresponds to the interaction term, which represents the effects of the earthquake after accounting for differences between groups and over time. The earthquake has no effect on concerns about infrastructure (column 1) but increases those about housing by 22 percentage points (column 2). Returning to the theory differentiating four types of affected citizens presented in section 2.2, we now have evidence to define them as *self-interested victims* who only prioritize their personal gains and are not more likely to focus on the distribution of collective goods.

2.6 Falsification Test

I also implement a falsification test for an effect we know to be absent (Keele, 2015). Specifically, I compare the matched samples from the surveys implemented six months and three months before the earthquake. I use a difference-in-differences strategy where "post" is the survey conducted three months before the earthquake and "treatment" is living in a county that would be exposed to the earthquake in three or six months. We would expect the interaction term to not be significant because we are comparing pretreatment surveys.

Table 2.6: Regression results

	Priorities	
	Infrastructure	Housing
	(1)	(2)
Treatment*Post (β_3)	−0.005	0.011
	(0.016)	(0.066)
Observations	636	636

Variables not shown: treatment, post, placebo covariates, and county fixed effects.
*p<0.1; **p<0.05; ***p<0.01

The results show that there is no substantive nor significant distinction between these

two surveys. Thus, if there is a post-treatment difference between groups, it can be attributed to the 2010 earthquake.

2.7 Robustness Check

If disaster victims are not more likely to focus on infrastructure, we might also expect them to not be more likely to pay attention to education and health either, two public goods directly connected with the consequences of the disaster. The earthquake damaged and destroyed schools and hospitals across the regions affected. Table 2.7 reports the β_3 coefficient when using education and health as outcomes.

Table 2.7: Regression results

	Priorities	
	Education	Health
	(1)	(2)
Treatment*Post (β_3)	0.027	−0.072
	(0.069)	(0.083)
Observations	954	954

Variables not shown: treatment, post, placebo covariates, and county fixed effects.
*p<0.1; **p<0.05; ***p<0.01

The disaster has not changed victims' focus on education or health in a substantive or significant way (interaction term), despite the massive destruction of public infrastructure. These findings provide extra evidence about affected citizens' myopia and self-interest.

2.8 Traditional Approach

How different are the results without using matching to reduce imbalances across time? Table 2.8 replicates the main results but without using a synthetic panel.

Table 2.8: Regression results

	Priorities	
	Infrastructure	Housing
	(1)	(2)
Treatment*Post (β_3)	−0.010	0.147***
	(0.017)	(0.045)
Observations	1776	1776

Variables not shown: treatment, post, placebo covariates, and county fixed effects.
*p<0.1; **p<0.05; ***p<0.01

As in the main results, the earthquake has a substantive and significant effect on housing but not on infrastructure. There is an important difference, however, between the results produced using the synthetic panel and the original survey data: in the latter, the effect of the earthquake on housing is smaller, decreasing from 22 to 15 percentage points. As a consequence, it is possible that in cases where the treatment does not have as large an effect as in this study, imbalances might lead to biases that can actually affect the interpretation of the evidence. In other words, the difference between both analyses could be more problematic in cases with smaller treatment effects.

2.9 Conclusions

Concerns about the intensity and frequency of natural disasters have increased in recent years, in particular due to studies that connect catastrophes with global warming (Lippsett, 2012; Van Aalst, 2006). Even though earthquakes are not associated with climate change, this study of the 2010 earthquake in Chile provides us with lessons to better understand victims' political preferences after other types of disasters such as floods, hurricanes, and tropical storms. All of these events have a key commonality: they diminish residents' living conditions and, therefore, will change their top priorities.

The 2010 earthquake in Chile provides an opportunity to learn about the political consequences of natural disasters through a carefully designed observational study. Because all regions in the country have been affected by this kind of natural catastrophe, all residents are eligible to be exposed to an earthquake. This feature increases comparability between affected and unaffected people. I construct a synthetic panel to be able to use multiple surveys and to implement a difference-in-differences strategy. Additionally, I use elements of design sensitivity literature to construct a study that is less likely to be affected by unmeasured covariates. Specifically, I focus on extreme treatment conditions to achieve that goal (Rosenbaum, 2004, 2011; Zubizarreta, Cerdá, and Rosenbaum, 2013). In an observational study, the findings will have more credibility when they are based on serious attempts to reduce and assess the impact of hidden biases (Rosenbaum, 2006).

The main findings show that affected citizens are self-interested since they are not more

likely to cite infrastructure as one of the most significant problems in their country. This null effect is surprising because the earthquake in question caused massive damage to public infrastructure and transportation and survey respondents had the chance to select their three top priorities. On the contrary, disaster victims are 22 percentage points more likely to highlight housing as a concern. These results speak directly to recent evidence that re-evaluates the relevance of voters' egotropic economic concerns (Murillo and Visconti, 2017; Visconti, 2017) when making electoral decisions in Latin America.

These findings are novel evidence of the political consequences of natural disasters. The extant literature tends to focus on how voters evaluate incumbents after such events. If citizens have new priorities, however, these might also affect their electoral choices. The prioritization of housing might be an important causal mechanism that allows us to better understand disaster victims' electoral preferences. The salience of new issues, like housing, might make victims more likely to vote for candidates associated with those concerns (Bélanger and Meguid, 2008). Therefore, affected voters might not focus on the evaluation of incumbent performance. If they are particularly concerned about the reconstruction of their houses, they should also select politicians connected with the promotion of social and welfare policies such as new housing.

2.10 Appendices

Appendix A: Fine Balance Before Matching

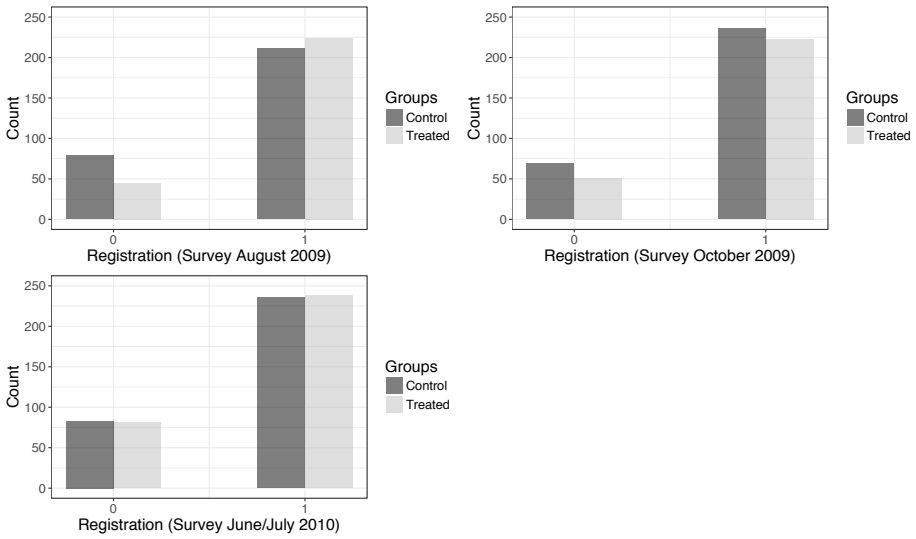


Figure 2.4: Registration

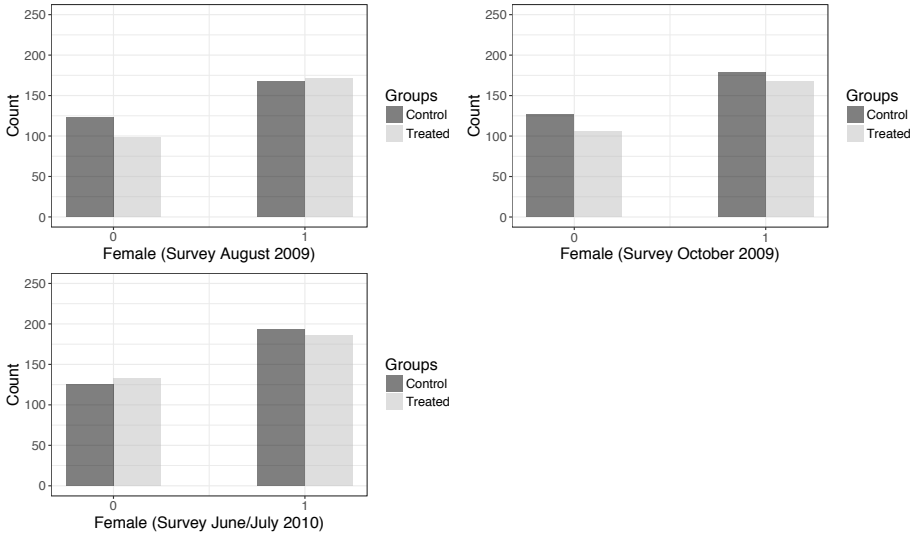


Figure 2.5: Gender

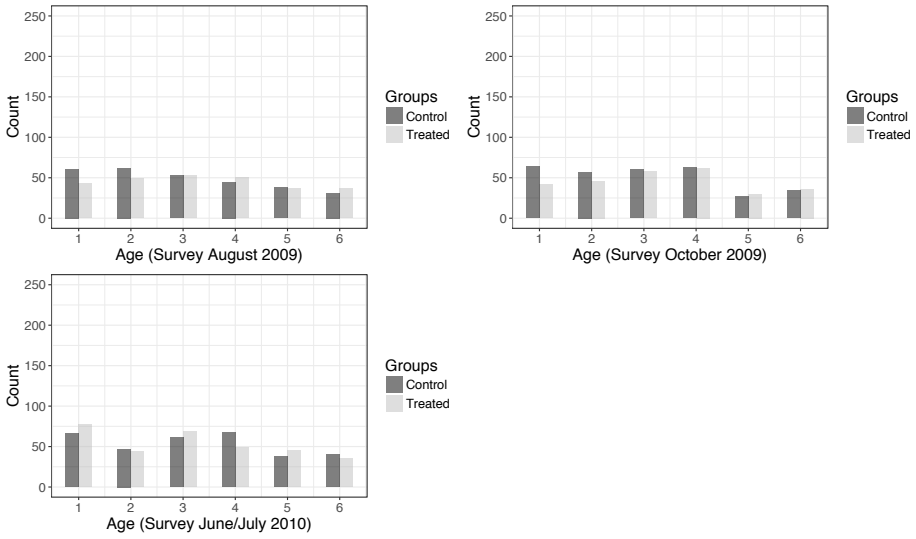


Figure 2.6: Age

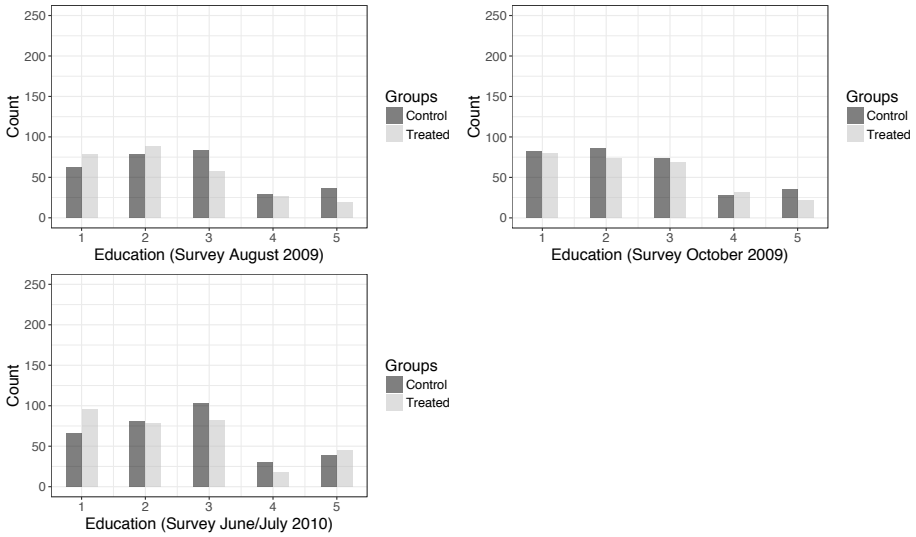


Figure 2.7: Education

Appendix B: Fine Balance After Matching

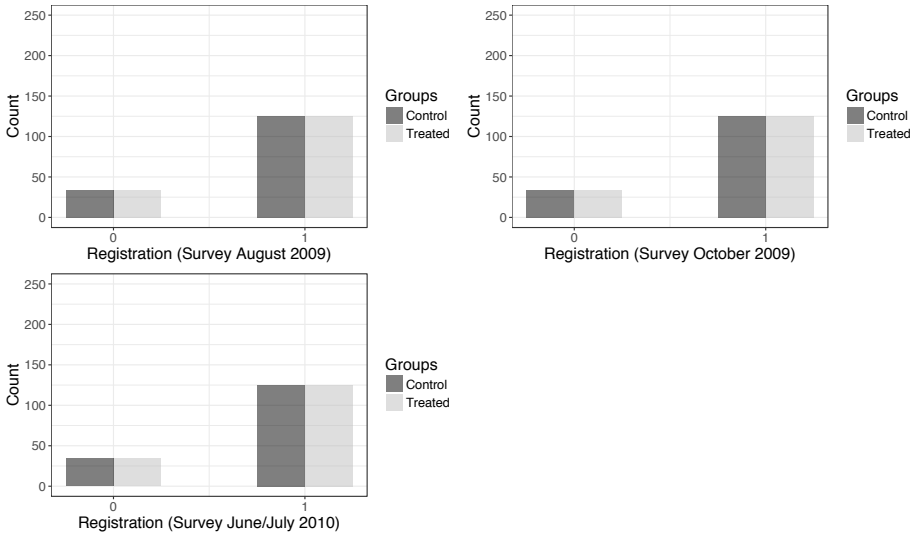


Figure 2.8: Registration

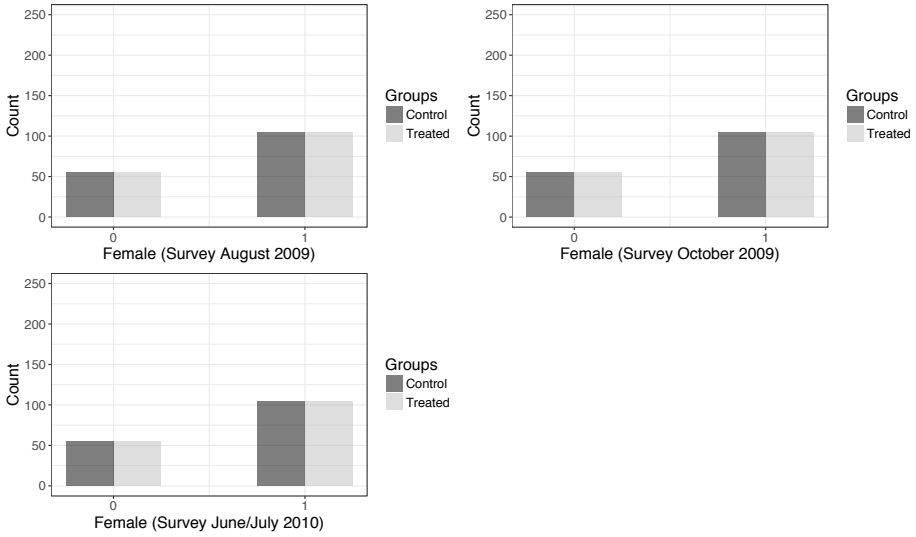


Figure 2.9: Gender

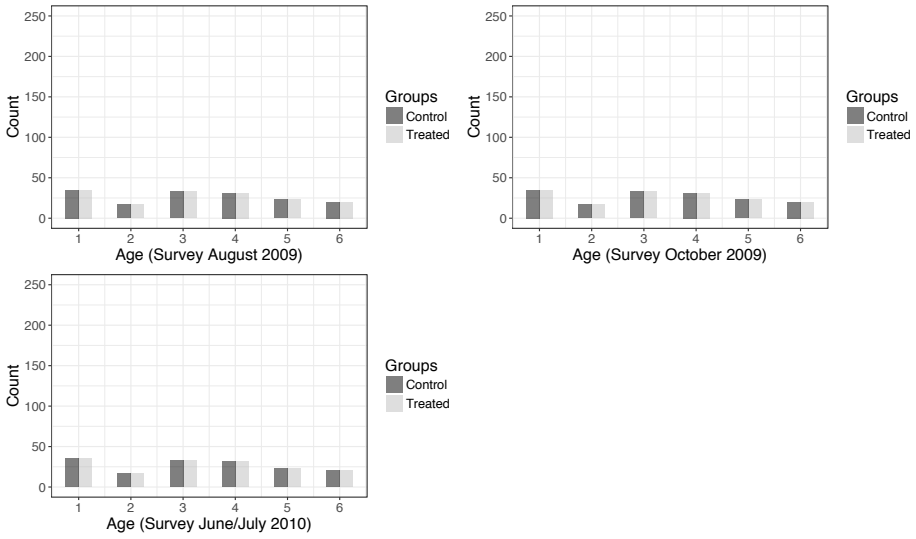


Figure 2.10: Age

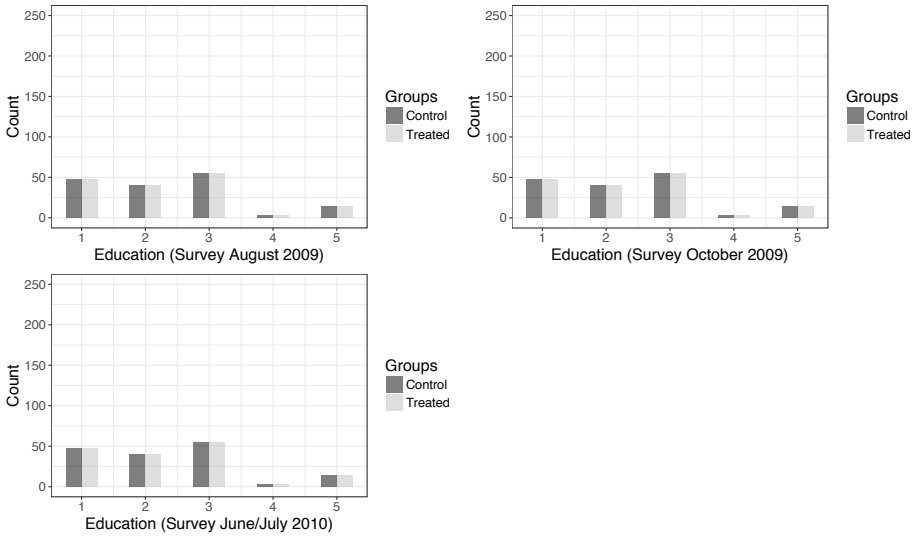


Figure 2.11: Education

Appendix C: Imbalances on Age

Table 2.9: Mean age (before matching)

Survey	Treated	Control
August 2009	3.372	3.103
October 2009	3.365	3.111
June/July 2010	3.144	3.263

Table 2.10: P-value age (before matching)

Surveys	Treated	Control
August 2009 - Survey October 2009	0.960	0.952
Survey August 2009 - Survey June/July 2010	0.096	0.231
Survey October 2009 - Survey June/July 2010	0.099	0.245

Table 2.11: Mean age (after matching)

Survey	Treated	Control
August 2009	3.314	3.314
October 2009	3.314	3.314
June/July 2010	3.314	3.314

Table 2.12: P-value age (after matching)

Surveys	Treated	Control
August 2009 - Survey October 2009	1	1
August 2009 - Survey June/July 2010	1	1
October 2009 - Survey June/July 2010	1	1

Appendix D: Imbalances on Education

Table 2.13: Mean education (before matching)

Survey	Treated	Control
August 2009	2.323	2.652
October 2009	2.423	2.502
June/July 2010	2.492	2.671

Table 2.14: P-value education (before matching)

Surveys	Treated	Control
August 2009 - Survey October 2009	0.339	0.154
August 2009 - Survey June/July 2010	0.108	0.852
October 2009 - Survey June/July 2010	0.518	0.096

Table 2.15: Mean education (after matching)

Survey August 2009	2.352	2.352
Survey October 2009	2.352	2.352
Survey June/July 2010	2.352	2.352

Table 2.16: P-value education (after matching)

Surveys	Treated	Control
August 2009 - Survey October 2009	1	1
August 2009 - Survey June/July 2010	1	1
October 2009 - Survey June/July 2010	1	1

Appendix E: Descriptive Statistics Before Matching

Table 2.17: Descriptive statistics before matching

Statistic	N	Mean	St. Dev.	Min	Max
Education	1,776	2.52	1.27	1	5
Age	1,776	3.22	1.63	1	6
Gender	1,776	0.60	0.49	0	1
Register	1,776	0.77	0.42	0	1

Appendix F: Descriptive Statistics After Matching

Table 2.18: Descriptive statistics after matching

Statistic	N	Mean	St. Dev.	Min	Max
Education	954	2.35	1.18	1	5
Age	954	3.31	1.67	1	6
Gender	954	0.65	0.48	0	1
Register	954	0.79	0.41	0	1

Chapter 3

Policy Preferences after Crime Victimization: Panel and Survey Evidence from Latin America

Abstract

In multiple countries, policy preferences tend to be explained by citizens' partisanship. Can these preferences be modified by particular negative events, such as being a crime victim? It is difficult to assess the political effects of crime, mainly because of the presence of unmeasured confounders. I use panel data from Brazil and strategies for reducing sensitivity to hidden biases to study how crime victims update their policy preferences. Additionally, I use survey data from 18 Latin American countries to improve the external validity of the findings. I show that crime victims are more likely to support strong-handed measures to reduce crime, such as allowing state repression, but without modifying their party identification. This reveals that (i) crime can change what people think the state is allowed to do, which can have important political implications; and that (ii) citizens can have flexible policy preferences that are not necessarily shaped by their partisanship.

3.1 Introduction

In multiple countries, citizens' policy preferences tend to be linked to their party identification or ideological disposition. For example, left-wing voters are more likely to support welfare policies (Shapiro, 2009) and redistribution (Alesina and Giuliano, 2009), while right-wing citizens have a higher probability of focusing on crime issues (Mayer and Tiberj, 2004). Yet there is evidence that short-term events, such as terrorist attacks and unemployment, can affect citizens' policy preferences (Berrebi and Klor, 2008; Margalit, 2013). This chapter answers two different questions. First, can crime victimization increase support for strong-handed or iron-fist policies to reduce crime, such as allowing state repression? Second, if the answer to the previous question is affirmative, is crime victimization modifying voters' policy preferences by changing their party identification or through a different causal mechanism?

The application of strong-handed measures to fight crime is associated with military policing and the erosion of procedural guarantees (Holland, 2013). Accordingly, these policies represent a statement about what the state can and cannot do to provide greater security. Studying citizens' preferences regarding crime is particularly important in contexts where delinquency is common, where politicians may exploit populist strategies to improve their electoral performance, and where the police have been involved in human rights abuses.

Latin America is one of the most violent regions in the world (UNODC, 2013). 43 of the world's 50 most dangerous cities are located in Latin America, even though this region represents less than 8% of the world's population (Magaloni, Franco, and Melo, 2015). As a consequence, survey respondents tend to highlight crime as one the most critical issues their countries face (Perez, 2015). The fear of crime has boosted the popularity and support of politicians who base their platforms on tough measures to combat crime (Azpuru, 2003). These iron-fist policies have been implemented in different Latin American countries, and can take the form of extralegal detention, arbitrary punishment, and the military-style occupation of entire neighborhoods (Dammert and Malone, 2006).

The problems associated with crime are highly visible in the largest country in the

region, Brazil, where the homicide rate in 2006 was 29.2 per 100,000 inhabitants, making it the third most violent country in Latin America after El Salvador and Venezuela (Carreras, 2013). These statistics have not improved in recent years, and "no country in the world has more cities plagued by violent crime than Brazil" (Rapoza, 2016). In Rio de Janeiro alone, the increase of property crimes between 1995 and 2003 was 122 percent (Bergman, 2006). This social context of insecurity and violence has been exploited by populist candidates who promise to bring "authority" back when fighting crime; this pattern was evident in the 2016 local elections (Winter, 2016). The Brazilian military police have been associated with the perpetration of human right abuses and extrajudicial and summary executions (Huguet and Carvalho, 2008). More examples of police misconduct in Brazil include unwarranted searches, beatings, and torture (Arias, 2006; Magaloni, Franco, and Melo, 2015).

It is important to better understand the political effects of crime in violent regions like Latin America. Previous studies have shown that crime can decrease victims' support for democracy (Merolla, Mezini, and Zechmeister, 2013), increase political participation (Bateson, 2012), and undermine incumbents' share of the vote (Marshall, 2015). We do not know, however, much about whether crime can modify victims' policy preferences and what they think the state is allowed and not allowed to do to protect them.[1]

It is challenging to address this research question for four main methodological reasons. First, being a crime victim is not a random event. Particular social circumstances can be correlated with crime victimization, generating a serial victimization problem. In other words, previous crime victims might be more likely to be crime victims again. Consequently, when using survey data it is hard to know if victimization is a unique event in a respondent's life or a common negative situation (Bateson, 2012). This problem can introduce biases, since the previous treatment status can affect the outcome (e.g. serial victims might get used to crime). Second, there might be a reverse causality problem. People who want strong-handed policies might be more likely to report a crime as a way to increase crime statistics and push for the implementation of those policies. Third, in any observa-

[1]Bateson, 2012 mainly focuses on the impact of crime on political participation, but she also provides evidence about how crime correlates with support for vigilantism and authoritarianism.

tional study the presence of hidden biases is a significant issue. Victims and non-victims can differ across multiple unobserved characteristics. This is particularly true when we use a national sample and compare individuals from different cities and, therefore, from diverse socioeconomic contexts. Finally, and related to the previous issue, neighborhood effects can be crucial (Bateson, 2012). Some sectors or areas within a city might be more or less secure, affecting the probability of being a crime victim. This point is particularly salient when analyzing data from multiple countries or from diverse cities or states within a country. Crime has a very local nature, and neighborhood characteristics are hard to adjust for.

In this chapter I pay careful attention to study design to address each of these concerns. I use panel data from two cities in Brazil (Baker, Ames, and Renno, 2006; Baker et al., 2015) to compare crime victims and unaffected respondents. I focus on individuals who were not crime victims in the previous wave to decrease the problems associated with serial victimization and reverse causation. Additionally, I reduce sample heterogeneity to decrease sensitivity to hidden biases (Rosenbaum, 2005, 2011) by comparing citizens from the same neighborhoods.

I use recent developments in optimal matching and mathematical programming to generate comparable groups of victims and non-victims that are similar on 48 pretreatment covariates. When using matching, there can be concerns about pruning observations to achieve balance. Based on this issue, I construct the largest representative matched sample using the `designmatch` package for `R` (Zubizarreta and Kilcioglu, 2016). Put simply, the matched groups obtained are not only balanced, but also similar to the unmatched sample. Moreover, I use survey data from 18 Latin American countries to improve the external validity of the findings obtained using panel data.

I show that crime victims are 7 percentage points more likely to support strong-handed policies to reduce crime, such as state repression, than non-victims. A possible causal mechanism explaining these results is the lower support for democracy generated by direct exposure to crime. As a consequence, victims are more willing to tolerate strategies that imply the erosion of basic rights. A second possible mechanism is that voters are strategically supporting parties that are "tough on crime," and as a result, are updating their policy

preferences. The evidence shows that crime victimization undermines citizens' support for democracy but does not affect their partisanship. Victims keep their party identification but have flexible policy preferences.

Voters' willingness to accept non-democratic measures, such as repression, can have critical consequences for the quality of democracy. Support of iron-fist policies can inform politicians about citizens' tolerance for human right abuses by the state. This issue becomes even more relevant because voters' policy preferences can actually shape the adoption of policies (Brooks and Manza, 2008; Lupu and Pontusson, 2011). Consequently, understanding the factors that influence citizens' policy preferences regarding crime is crucial.

This article provides four main contributions to the existing literature. First, it adds to a growing body of research that studies the political effects of crime (Bateson, 2012; Kronick, 2014; Marshall, 2015); and in particular, it focuses on support for iron-fist policies. They delineate the limits of the state and what it is allowed to do to ensure public security. Therefore, it is crucial to understand the factors explaining voters' support for these measures. Second, it provides novel evidence about the causal mechanisms explaining voters' new policy preferences. In particular, it shows how the reduction of democratic values among victims might lead them to support particular measures such as repression. Third, it dialogues with studies of how negative events can affect voters' policy preferences. Negative shocks, such as crime, economic crises, and natural disasters, are common situations in the developing world, and constantly deteriorate citizens' living conditions. For example, voters might also update their policy preferences after being a disaster victim, but probably in a different direction. Finally, it contributes to the discussion about the importance of study design for reducing sensitivity to unmeasured factors and model dependence.

3.2 Crime Victimization and Political Outcomes

Crime victimization has clear psychological effects on victims, such as increasing their levels of anger, fear, and sadness (Greenberg and Ruback, 2012). However, it can also have important political and electoral implications.

A significant number of studies have attempted to determine if crime affects incum-

bents' share of the vote. According to the theory of retrospective voting, crime victims will sanction the government in the consecutive elections. Similar arguments have been used to study how economic conditions affect voters' electoral decisions, and if citizens reward or sanction incumbents based on economic perceptions (Lewis-Beck and Stegmaier, 2000, 2007). There is mixed evidence regarding the effects of crime on aggregate electoral results. Cummins, 2009 analyzes gubernatorial elections in the US from 1986 to 2004. He finds that crime has a large impact on state but not on national elections, and that this effect is greater in states with a more educated population.

In the case of Latin American countries, Marshall, 2015 shows that voters punish the government for local homicides in Mexico, depending upon whether they consume information. Conversely, Perez, 2015 finds, using survey data from the AmericasBarometer, that crime victimization does not affect voters' electoral decisions; however, perceptions of high levels of insecurity do impact respondents' political choices. Kronick, 2014 attempts to reconcile these mixed findings, showing that incumbents can escape electoral punishment under particular circumstances. External factors can decrease political authorities' ability to manage crime. For example, the counternarcotics operations in Colombia had a spillover effect in Venezuela. The author finds that previous to this episode, Venezuelan voters held politicians accountable based on changes in local homicide rates, but during the operations in Colombia, voters stopped punishing incumbents because the origin of the negative events could not be attributed to them.

A natural extension of studying the electoral impact of crime is exploring its effects on political participation. Bateson, 2012 argues that crime victims tend to engage more in political and civic activities than non-victims. Using survey evidence from five continents, she shows that the impact of crime victimization on political participation can be compared to five to ten additional years of education. However, Trelles and Carreras, 2012 provide a different finding using data from Mexico: they show that criminal violence reduces turnout. Crime victims tend to abandon public participation, such as voting in elections. The authors offer two possible explanations for this result: either victims may be disenchanted with the political system or they may not willing to risk their personal safety by participating in public places. Berens and Dallendörfer, 2017, in contrast, argue that the impact of crime

on political participation is conditional to the level of violence.

Crime victimization can also undermine support for and the legitimacy of democracy. This negative link has been supported by multiple studies. Carreras, 2013 shows that victimization and high perceptions of violence have a negative impact on support for democracy in Latin America. Fernandez and Kuenzi, 2010 find a similar negative correlation between perceptions of public safety and attitudes toward democracy in the region. In a similar vein, Malone, 2010 studies how crime affects support for the rule of law in Central America. Finally, Merolla, Mezini, and Zechmeister, 2013 provide survey and experimental evidence showing that crime reduces support for democracy in Mexico.

The literature has paid less attention to how crime can modify citizens' policy preferences. Krause, 2014 studies the link between crime news and support for authoritarian measures in Guatemala. She finds that news about crime reduces trust in government, which increases support for authoritarian strategies of controlling crime. However, this study focuses on the effects of exposure to the news but not on the direct consequences of crime victimization.

In summary, there is systematic evidence about how crime victimization can yield different political outcomes. However, the literature has paid less attention to how this type of negative event can modify victims' policy preferences and what they think the role of the state is in fighting crime. Furthermore, most of the literature based on survey evidence has not adequately addressed relevant endogeneity concerns. For example, because political preferences can influence voters' perceptions of insecurity, the literature might be overstating the political impact of these perceptions. To circumvent this issue, I focus on crime victimization, which should be less endogenous to respondents' electoral choices. Moreover, another problem when using survey data is that the treatment and covariates are measured at the same time, which can lead to potential post-treatment biases. The use of panel data can help address this previous issue.

3.3 Crime Policy Preferences

Crime-reduction policies can adopt one of two main approaches. The first is based on social policies and emphasizes treatment and rehabilitation, while the second sees crime as a concrete problem that can be solved with effective and strong actions (Estrada, 2004).[2]

Iron-fist or strong-handed policies can be associated with the latter approach. They represent different direct and tough measures to reduce and fight crime: for example, increasing discretionary rules to detain suspects and militarizing policing. These strategies are a radical form of "penal populism," and in general imply greater repression and the deterioration or dilution of procedural rights (Holland, 2013).

Support for these measures can have crucial political implications, because they refer to the limits of the state's power when fighting crime, and in particular to the boundaries that cannot be transgressed in the attempt to increase security. Moreover, state repression can affect citizens' human rights and erode democratic institutions. The inviolability of citizens' bodily integrity is a basic principle in contemporary democracies that can be undermined by the implementation of iron-fist policies (Fuentes, 2005). In multiple countries in Latin America the state is the main actor involved in human rights violations due to the implementation of military strategies to fight crime (Cruz, 2010).

In contexts of high crime rates, it becomes important to understand whether victimization makes citizens more or less likely to support these different policy approaches. What explains the support for tougher crime-fighting measures? Prior research suggests two main explanations for citizens' attitudes toward these particular policies. The first relies on voters' ideological preferences and/or party identification. The second focuses on how specific circumstances, such as a change in media coverage, can shape voters' policy preferences.[3]

Regarding the first explanation, right-wing voters are more likely to care more about

[2]Of course, we can also understand these two approaches as a continuum from a total focus on rehabilitation to a total focus on repression.

[3]Holland, 2013 also mentions a third factor: the role of public opinion in shaping preferences towards strong-handed policies. However, it is possible to merge that third variable with the second one (i.e. how specific circumstances shape policy preferences).

crime than left-wing voters (Mayer and Tiberj, 2004). In a similar vein, Gerber and Jackson, 2016 show that right-wing authoritarianism can predict support for punitive measures. Furthermore, the policies that emphasize punitive sanctions tend to be associated with conservative rather than liberal politicians. For example Republican former US president Ronald Reagan summarized his views about how to fight crime by declaring that "here in the richest nation in the world, where more crime is committed than in any other nation, we are told that the answer to this problem is to reduce our poverty. This isn't the answer (...) [The] government's function is to protect society from the criminal, not the other way around" (Beckett, 1999, p.48). Moreover, there is evidence in the US that the proportion of Republican legislators is correlated with imprisonment rates at the state level (Beckett and Western, 2001).

The link between ideology and crime policies is also evident in Latin America. Right-wing candidates in Honduras, Mexico, and Peru have promoted strong-handed policies to combat crime (Cohen and Smith, 2016). In El Salvador, the conservative party ARENA attempted to boost its support in a context of high crime rates by implementing iron-fist policies, such as diluting due process guarantees (Holland, 2013). In the case of Brazil this pattern is also clear, as in the case of the right-leaning former governor of the state of Rio de Janeiro, Marcello Alencar. Alencar decided to provide semi-automatic weapons to the police and to implement a "bravery bonus" to officers who engage in violent confrontations (Magaloni, Franco, and Melo, 2015). In summary, right-wing politicians can be linked with these kind of measures to combat crime. Right-wing citizens, similarly, are more likely to support tougher measures to reduce crime and to focus less on social policies.

Nevertheless, it is hard to believe that voters have static policy preferences, particularly when they are exposed to adverse conditions that might affect their priorities and primary concerns. These include negative events that deteriorate victims' living conditions, a common situation in the developing world. For example, Latin American voters are vulnerable to income shocks generated by economic volatility (Murillo and Visconti, 2017), high crime rates (Carreras, 2013), and natural disasters (Charvériat, 2000). These adverse conditions might affect the policies citizens would like to see implemented. However, a negative event such as crime victimization can modify victims' policy preferences through

different causal mechanisms. In this chapter I provide empirical evidence to support the argument that voters have flexible political preferences that respond to their personal circumstances, and to illuminate the causal mechanisms connecting crime and preferences for strong-handed policies.

I argue that crime victimization can have substantive and meaningful effects on victims' policy preferences. This can happen through two different causal mechanisms. First, it can change the value attached to democracy. Second, it can alter voters' partisanship (only if we understand partisanship as a running tally and not as a political identity).

The first mechanism is based on consistent evidence showing that crime can affect victims' democratic values and support for the rule of law (Carreras, 2013; Krause, 2014; Merolla, Mezini, and Zechmeister, 2013). Crime can undermine the legitimacy of the political system (Cruz, 2010) and increase support for a radical change (Seligson and Azpuru, 2000). In fact, fear of crime has been connected with support for regimes that reduce civil liberties (Pérez, 2003). Additionally, there is evidence of a correlation between democratic preferences and support for policies that protect citizens' due process rights (Seligson, 2003). Consequently, a lower attachment to democratic values might explain why crime victims might be willing to accept the erosion of some basic rights in favor of more punitive measures to combat delinquency in their countries. Civil liberties are directly linked to democratic values and the rule of law, and direct exposure to crime can increase victims' willingness to sacrifice these rights. Simply put, the causal mechanism that connects victimization with the new policy preferences is the lower value attached to the democratic system by victims.

The second mechanism is based on the conceptualization of partisanship as a running tally. Citizens may change their partisanship based on which party will benefit them more (Achen, 1992). In other words, party identification can be understood as the result of a rational calculation by voters (Fiorina, 1981). In particular, citizens exposed to crime might think that a specific party can better address their main concerns, and will update their partisanship accordingly.

Historically, party identification has not been relevant for explaining electoral behavior in Brazil (Ames, 2001), where the party system was candidate instead of party-centered

(Mainwaring, 1999). However, that political context has changed in recent decades. For example, Samuels and Zucco (2014, p.2) use survey experiments to study partisanship in Brazil, and find that "exposure to party cues strongly shapes voter opinion." Additionally, Lupu (2015, p.244) uses panel data from Brazil in 2002 to show a "consistent causal effect of partisanship on vote intentions." Lupu (ibid., p.228) also uses survey data from AmericasBarometer to provide "evidence that patterns of partisanship in Latin America closely resemble those in advanced democracies." Therefore, Latin America is not a region lacking of partisanship.

Since some parties in Brazil have clear platforms regarding crime, we would expect certain voters to connect party labels with policy outcomes. For example, in 2006 the PSDB presidential candidate, Geraldo Alckmin, focused his platform on topics related to public security and promoted strong-handed policies to combat crime (Ayllón and García, 2006). As a consequence, parties like the PSDB might become more attractive to crime victims.

Party identification can also be understood as a form of social group identification, like religion and social class (Green, Palmquist, and Schickler, 2002). In this case, voter attachment to parties should persist over time (Campbell et al., 1960). According to this understanding, a particular crime event should not affect voters in such a deep way as to modify their identities or how they define themselves politically. Results showing that voters are changing their policy preferences but not their party identification can be read as evidence of partisanship being a political identity in Latin America.

Consequently, I will test the impact of crime on the main outcome (i.e. policy preferences) and on two possible causal mechanisms (i.e. support for democracy and partisanship).[4] I expect to find a substantive and significant effect of crime victimization on policy preferences. I hypothesize that this change is explained either by a lesser degree of support for democratic values or by a new party identification.

The study of negative events has been dominated by a retrospective voting approach, whose most common prediction is that victims will punish incumbent candidates. In this

[4]This strategy is called a *single-experiment approach* because both the outcome and the mechanisms are captured within the same study (Imai et al., 2011).

chapter, however, I focus on the prospective dimension of voters' decisions by paying attention to the policies they most care about after crime victimization: in particular, support of state repression.

3.4 Research Design

Random assignment is the best strategy for establishing the causal effect of a particular intervention, because treatment assignment is independent of potential outcomes (Morgan and Winship, 2014), and in expectation, observed and unobserved covariates should have similar distributions between treatment and control groups (Bowers, 2011). However, randomization is not always feasible for ethical or practical reasons. The alternative strategy for studying a phenomenon that cannot be randomized, such as crime victimization, is a well-designed observational study structured to resemble a simple randomized experiment (Rosenbaum, 2010), and to use elements from the design-based approach to improve the study design (Keele, 2015). These include focusing on endogeneity (Imbens, 2010), not including final outcome data (Rubin, 2008), and not relying on statistical modeling (Keele, 2015).

What makes an observational study good? Following some of the recommendations provided by Rosenbaum, 2010, 2011: first, the treatment should be well-defined. This means that we know when it starts and therefore what the pretreatment and post-treatment covariates are. Second, even though there is no random assignment, the intervention should seem haphazard or not obviously related to potential outcomes. Third, treated and control groups should be comparable: in other words, the distributions of observed covariates should be similar across both groups. Fourth, the design should make use of strategies for reducing sensitivity to unobserved biases, such as decreasing unit heterogeneity. I apply these four previous criteria in the design of this observational study.

Regarding the first recommendation, the main problem when working with survey data is the lack of pretreatment covariates, since adjusting for post-treatment characteristics can introduce biases (Rosenbaum, 1984). Therefore, I use panel data from Brazil collected between 2002 and 2006 (Baker, Ames, and Renno, 2006; Baker et al., 2015) to adjust only

on covariates captured in waves before respondents were victimized by crime. The survey questionnaire asked a standard battery of questions about political preferences, demographics, media exposure, crime victimization, feeling thermometers, and social networks.[5] The panel structure allows me to include pretreatment measures of the outcomes, the oldest and most basic tool for reducing the ambiguity of the effect of a treatment in an observational study (Rosenbaum, 2015a).

Second, though crime victimization is not randomly assigned, it is possible to exploit certain aspects of the study design to make this situation more haphazard. In particular, I only select respondents that in the wave t were not affected by crime. Then, if in wave $t+1$ they were crime victims, they are incorporated into the treated group, and if they keep being non-victims they go into the control. Consequently I exclude by design citizens who are serial victims of crime.

The third recommendation emphasizes the need to compare similar groups of exposed and unexposed individuals. I construct these groups by using an optimal matching algorithm that finds the largest representative pair-matched sample that is balanced by design (Zubizarreta and Kilcioglu, 2016). I explain the details of this technique later.

The fourth strategy focuses on decreasing sensitivity to hidden biases by reducing the heterogeneity of the sample. As Rosenbaum, 2005 shows, reducing unit heterogeneity implies that larger unobserved biases will be needed to explain away a particular effect. A good example of this strategy are the studies based on identical twins (see Ashenfelter and Rouse, 1998). Consequently, in an observational study it is preferable to focus on more homogeneous and comparable subsets (Keele, 2015) or on natural blocks (e.g. neighborhoods), since unmeasured covariates should be more similar between treated and control groups (Pimentel et al., 2015). The use of national surveys does not help achieve this goal, because they increase the heterogeneity of the sample. Consequently, I exploit the design of the panel data since it focuses only on two mid-sized cities in Brazil: Juiz de Fora in the state of Minas Gerias and Caxias do Sul in Rio Grande do Sul. Both cities have similar characteristics, such as the size of the electorate, their educational and income levels, and

[5] See the supplementary appendix and Baker et al., 2015 for more details about this panel survey.

racial composition (Baker, Ames, and Renno, 2006).[6] According to the unmatched sample, they also have similar crime rates in wave $t+1$: 15% of respondents were crime victims in Juiz da Fora, and 14% in Caixas do Sul. Additionally, the data provides neighborhood indicators, which allows me to achieve balance in terms of respondents' geographic location.

How does one go about building a group of affected and unaffected citizens that are balanced in their observed characteristics? One alternative is matching, which attempts to generate a treated and control group with similar covariate distributions (Ho et al., 2007; Stuart, 2010). However, traditional matching techniques, such as propensity score and Mahalanobis distance, do not guarantee covariate balance and in some occasions can even make balance worse across observed covariates (Sekhon, 2009). These methods often involve a process of manually iterating the model until covariate balance is obtained (Hainmueller, 2011). Moreover, a possible concern when using any type of matching technique is that it requires some level of pruning to obtain balance. This means that the matched sample might be different than the unmatched sample.

In the attempt to address these limitations, I use the `designmatch` package developed by Zubizarreta and Kilcioglu, 2016, which allows me to find the largest representative sample that achieves covariate balance. This algorithm maximizes the size of the sample that: (i) meets the balance requirements defined beforehand and (ii) is similar to a target sample also defined beforehand (in this case the unmatched sample). Point (i) addresses the limitations of traditional matching techniques because the algorithm directly balances the original covariates without needing to estimate a propensity score. Point (ii), furthermore, means that the samples before and after matching are similar, making pruning less of a concern.

I use mean balance constraints for 47 covariates. The algorithm matches individuals such that the treated and control matched groups cannot differ in their means by more than 0.1 standard deviation from the unmatched sample. As a consequence, the standardized differences between the matched treated and control group cannot be larger than $0.1 * 2$ standard deviation. In other words, the standardized differences between the matched groups

[6] They are different in terms of strength of political parties and salience of ideological cleavages (Baker, Ames, and Renno, 2006).

cannot be larger than twice the standardized differences between the matched sample (i.e. both matched groups) and the unmatched sample (see ibid. for more details).

All of the mean balanced covariates are ordinal or binary; thus, adjusting their means is a meaningful decision.[7] I also use fine balance for neighborhood, which implies that both groups will have the same frequency for this covariate but without restricting who is paired with whom (Rosenbaum, Ross, and Silber, 2007; Zubizarreta, 2012). Therefore, I am adjusting for a total of 48 different observed covariates.[8]

In the matching procedure I include covariates that can affect both the treatment assignment and the outcome (Stuart, 2010). The full list is provided in Figure 3.1 and in the supplementary appendix, but some of the most relevant respondent characteristics are age, education, gender, ideology, job in the formal sector, media consumption, partisanship, policy preferences, political knowledge, race, and religion. All of these are pretreatment covariates.

The treatment is a binary indicator for being a witness or victim of crime[9] in wave $t+1$ (only among a group of respondents who were not witnesses or victims of crime in wave t). The question used to construct the treated and control groups is the following: "Have you been a witness or a victim of crime in the past 12 months? This includes crimes such as assault, robbery, or aggression." Unfortunately, the question does not differentiate between different types of crimes.

The main outcome is a binary indicator of support for the use of strong-handed measures and repression to reduce crime (wave $t+1$).[10] I also focus on the following outcomes to understand the causal mechanisms: (i) a binary indicator of support for democracy,[11]

[7] In the case of nominal covariates, it is advisable to use other forms of covariate balance (see Resa and Zubizarreta, 2016; Zubizarreta, 2012)

[8] See the supplementary appendix for details about the structure of the panel data, and the construction of covariates and outcomes.

[9] The treatment involves being a crime victim but also being a witness. Even though they are different events, both would be expected to affect voters in similar ways. In the worst case scenario, any effect can be seen as a conservative estimate.

[10] Support for the following statement: "The best way to reduce crime is with repression and an iron fist."

[11] Support for the following statement: "Democracy is always better than other forms of government."

(ii) a binary indicator of identification with the PT;[12] (ii) with the PMDB;[13] (iii) with the PSDB;[14], (iv) and with the PFL.[15]

To estimate the effect of crime victimization I use a linear regression with cluster standard errors at the neighborhood level:

$$Y_{it+1} = \alpha + \beta_1 T_{it+1} + \beta_2 P_{it} + \beta_3 X_{it} + \sigma_n + \varepsilon_i \tag{3.1}$$

Y is a binary indicator that represents the outcome of interest in wave $t+1$. T depicts the treatment (crime victimization in wave $t+1$), P describes a pretreatment measure of the outcome from wave t, and X corresponds to a set of pretreatment covariates that might predict the outcome (education and age). σ_n represents neighborhood fixed effects. I also provide the unadjusted estimates to increase transparency (Lin, 2013); this means no controls or fixed effects. Moreover, in the supplementary appendix I use a one-sided Wilcoxon signed rank test statistic as another method of inference since it is less dependent on distributional assumptions, and allows us to conduct the amplification of a sensitivity analysis for hidden biases (Rosenbaum and Silber, 2009).

3.5 Results Panel Data

The unmatched sample has 1916 subjects in the control group (not crime victims in wave t and $t+1$) and 320 in the treated group (not crime victims in wave t but crime victims in wave $t+1$). The matching algorithm will find the largest representative matched sample that fulfills the following criteria: (i) mean balance for 47 covariates between the matched and unmatched sample, (ii) mean balance for 47 covariates between the matched treated and control group, and (iii) fine balance for neighborhood between the matched treated and control group. After optimizing these criteria, the matched sample has 271 subjects in each group, which makes a total of 542 individuals that are similar to the 2236 subjects in the unmatched sample.

[12]PT: Partido dos Trabalhadores (Workers' Party).

[13]PMDB: Partido do Movimento Democratico Brasileiro (Brazilian Democratic Movement Party).

[14]PSDB: Partido da Social Democracia Brasileira (Brazilian Social Democracy Party).

[15]PFL: Partido da Frente Liberal (Liberal Front Party).

Figure 3.1 shows the standardized differences between the matched and unmatched samples (black dots), and between the matched treated and control groups (gray asterisks). By design, the first standardized differences cannot be larger than 0.1, and the second cannot be larger than 0.2 pooled standard deviations. The dotted lines represent the different tolerances for each comparison. To confirm covariate balance, the gray asterisks cannot be above the gray line, and the black dots cannot be above the black line. The figure shows how these balance requirements are met by default when using the `designmatch` package.

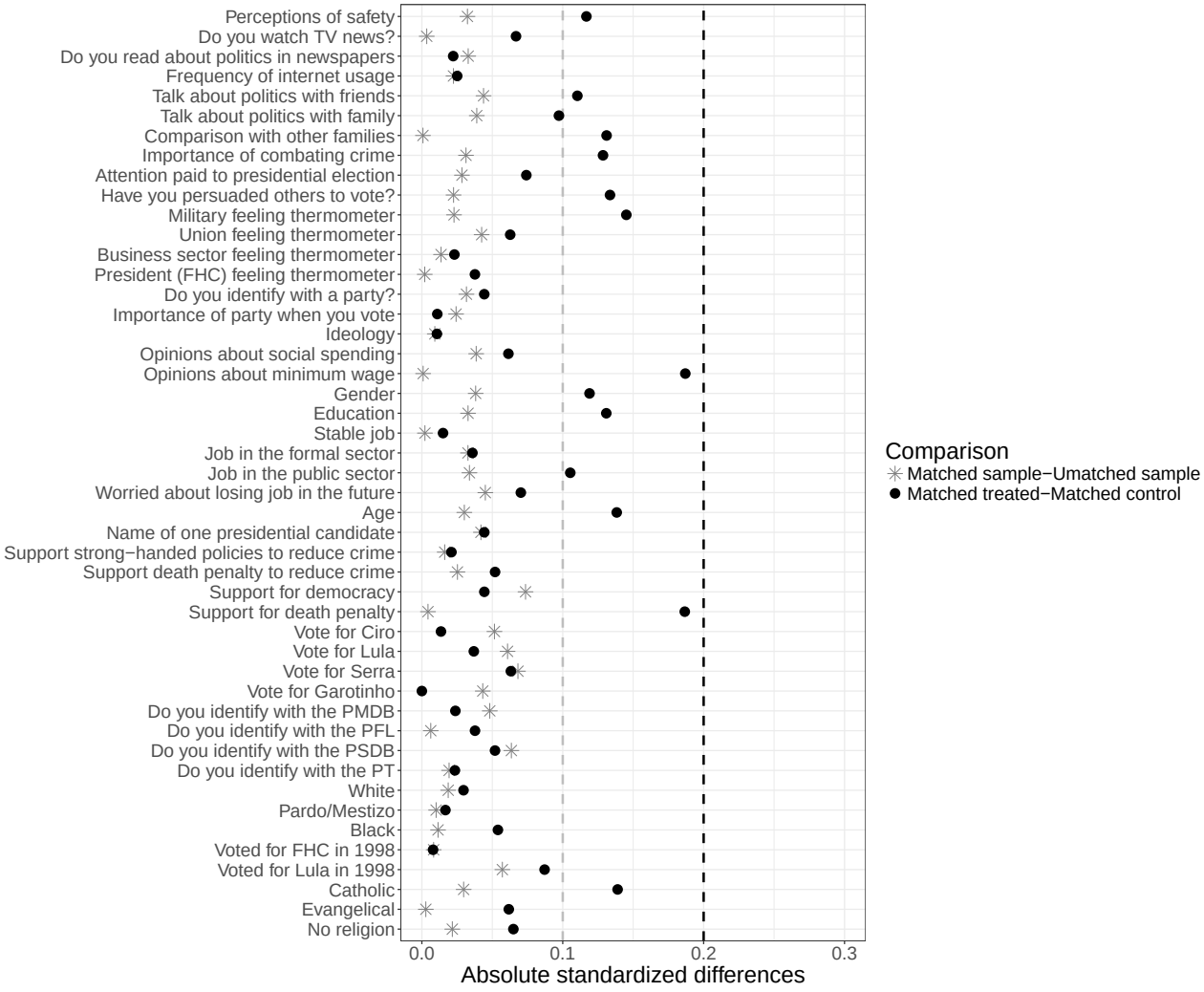
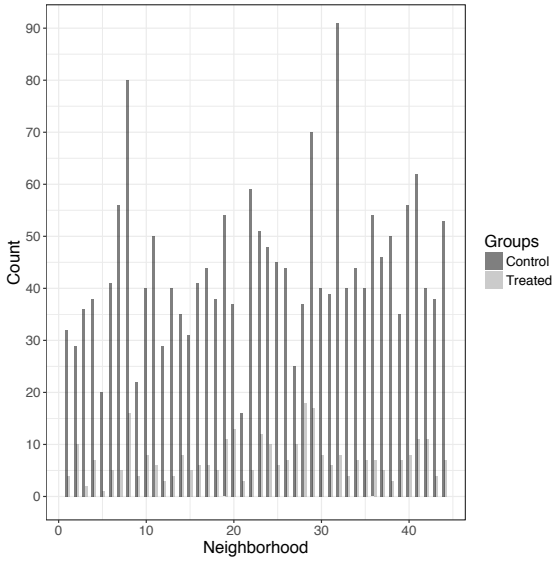


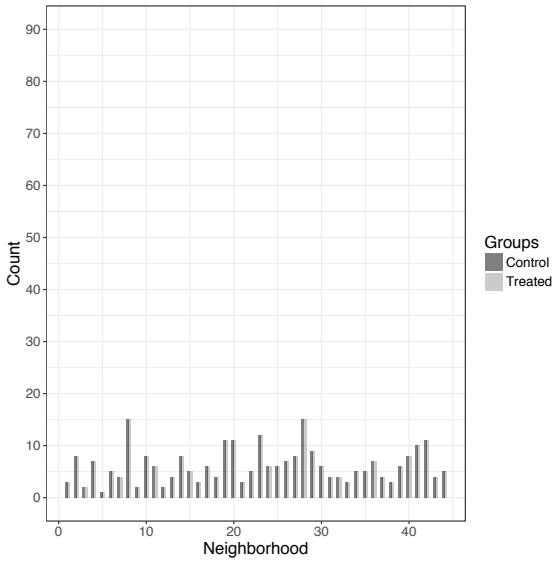
Figure 3.1: Mean balance

Additionally, I constrain the marginal distribution of neighborhoods using fine balance. This means that the treated and control groups will have the same number of subjects in

each neighborhood. However, this balance constraint does not focus on pairing.



(a) Before matching



(b) After matching

Figure 3.2: Fine balance for neighborhood

The main outcome of interest is a binary indicator of support for the following statement: "The best way to reduce crime is with repression and an iron fist." The treatment is to be a crime victim in wave $t+1$ conditional on not being a victim in wave t.

It is also important to confirm that party identification correlates with policy preferences

in Brazil, because the causal mechanism that voters will sympathize with the party more likely to benefit them can only be credible if parties are associated with policy outcomes. The evidence shows that parties are not irrelevant in Brazil. 47% of respondents in the matched sample identified themselves with one of the four main parties, and only 24% of participants in the matched sample said that candidates' party affiliation is not relevant for them when making electoral decisions. These numbers are congruent with evidence showing that partisanship has become more salient in this country in recent decades.

Table 3.1 reports the impact of crime victimization on policy preferences. Columns 2, 3, and 4 provide unadjusted estimates. Column 5 includes a pretreatment measure of partisanship[16] to check if it correlates with preferences about iron-fist policies.

Table 3.1: Regression results

	Strong-handed policies and repression to reduce crime (wave t+1)				
	(1)	(2)	(3)	(4)	(5)
Crime Victimization	0.070**	0.063**	0.069**	0.063**	0.073**
	(0.031)	(0.031)	(0.029)	(0.030)	(0.030)
PT (wave t)					0.012
					(0.031)
PSDB (wave t)					0.237*
					(0.131)
PMDB (wave t)					0.061
					(0.052)
PFL (wave t)					−0.159***
					(0.049)
Controls	Yes	No	Yes	No	Yes
Neighborhood fixed effects	Yes	Yes	No	No	Yes
Observations	542	542	542	542	542

Note: *p<0.1; **p<0.05; ***p<0.01

The results show that the treatment increases the chances of supporting strong-handed policies and repression after being a crime victim by 7 percentage points (column 1). 18% of victims support strong-arm policies, while 12% of non-victims have that policy preference. Here is crucial to remember that both groups are balanced on the pretreatment

[16] A binary indicator of sympathy for a particular party.

measure of this outcome (besides being balanced in 47 other covariates). These are important results because they represent a substantive effect on the understanding of what the state is allowed to do to protect citizens. These crime policy measures involve more than the implementation of a particular program or a budget increase; on the contrary, they directly imply the use of repression as a valid method for combating crime.

Column 5 reveals a significant and substantive correlation between sympathy toward the PSDB and PFL and support/nonsupport for iron-fist policies, and no evidence of correlation for the PT and PMDB. The PSDB can be associated with more conservative measures, while the PFL advocates for the protection of democratic rights in its ideological platform (Alcántara and Freidenberg, 2001). These findings tell to us that some parties might be clearly associated with platforms regarding how to deal with crime, and if partisanship is fluid, citizens might change their party identification in correspondence to being a crime victim.

What mechanism explains the impact of crime victimization on policy preferences? I hold that there are two main possibilities. First, crime might be reducing support for democracy, and making citizens more willing to tolerate repression and non-democratic practices. Second, voters might be changing their partisanship according to a running tally model. They will feel closer to the party most likely to benefit them, and consequently update their policy preferences.

Analysis of the causal mechanisms requires the untestable assumption that conditional on observed pretreatment covariates, the treatment assignment is independent of potential outcomes and potential mediators; and that conditional on the observed treatment and pretreatment covariates, the observed mediator is independent of potential outcomes (Imai, Keele, and Tingley, 2010; Imai et al., 2011). In the attempt to make this assumption plausible I only adjust the matching procedure for covariates captured before the treatment and for pretreatment measures of the outcome and mediators. Thus, both groups are balanced on the pretreatment measures of the possible causal mechanisms. Table 3.2 reports the effect of the negative shock on a binary indicator of support for democracy[17] and expressing sympathy for one of the main political parties in Brazil: the PT, PMDB, PSDB, or PFL.

[17]Support for the following statement: "Democracy is always better than other forms of government."

Table 3.2: Regression results

	Causal Mechanisms (wave t+1)				
	Democracy	PT	PMDB	PSDB	PFL
	(1)	(2)	(3)	(4)	(5)
Crime Victimization	−0.066*	−0.003	0.022	−0.005	0.003
	(0.039)	(0.034)	(0.028)	(0.012)	(0.012)
Controls	Yes	Yes	Yes	Yes	Yes
Neighborhood fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	542	542	542	542	542

Note: *p<0.1; **p<0.05; ***p<0.01

Though crime victimization does affect respondents' support for democracy, there is no evidence to confirm the idea that victims are changing their partisanship. This shows us that policy preferences are not necessarily always shaped by voters' party attachments, but rather can be modified by particular negative experiences. Citizens can have dynamic preferences, even though their partisanship is less flexible. In other words, victims' new policy preferences are mainly explained by their new attitudes towards democracy and not by a new party identification.

3.6 External Validity: Results Survey Data

Are these results a consequence of a particularity of the sample composition? Or of the year the survey was conduced? Is this pattern only present in Brazil? In an attempt to answer these questions, I use data from the Latin American Public Opinion Project (LAPOP) to study the correlation between crime victimization and policy preferences in 18 Latin American countries in the year 2012.[18] Since there is an evident trade-off between internal and external validity, this second study is less robust than the first because it is

[18]Support for strong-arm crime-reduction policies was not asked about in most of the countries in the most recent LAPOP survey conducted in 2014.

harder to reduce sensitivity to hidden biases without panel data. Nevertheless, it does help us check if similar results are obtained when we study all Latin American countries.

The main dependent variable is support for strong-arm policies.[19] I also test the effect of crime on the two mechanisms of interest: support for democracy,[20] and sympathy for the first, second, third, and fourth-most preferred party in each country.[21] To estimate the effect of crime victimization, I use a linear regression with cluster standard errors at the municipality level, and only include "placebo" covariates as controls. Covariates should not be affected by crime victimization, because that can introduce post-treatment biases. Therefore, I use the following four controls: age, education, gender, and ethnicity. I also include country fixed effects in the estimation. I do not use matching in this section to avoid any concerns about pruning observations since the main goal of this analysis is to improve external validity (see supplementary appendix for more detail).

$$Y_i = \alpha + \beta_1 T_i + \beta_2 P_i + \sigma_c + \varepsilon_i \tag{3.2}$$

Y is a binary indicator that represents the outcome of interest. T depicts the treatment (crime victimization), P describes the set of "placebo" covariates (age, gender, education, and ethnicity). σ_c represents country fixed effects. Table 3.3 displays the main results.

Table 3.3: Regression results

	Dependent variable:					
	Democracy	Party 1	Party 2	Party 3	Party 4	Strong-handed policies
	(1)	(2)	(3)	(4)	(5)	(6)
Crime Victimization	-0.013^{*}	-0.009	0.005	0.002	0.000	0.057^{***}
	(0.007)	(0.005)	(0.004)	(0.002)	(0.001)	(0.008)
Placebo covariates	Yes	Yes	Yes	Yes	Yes	Yes
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Countries	18	18	18	18	18	18
Observations	28,803	28,803	28,803	28,803	28,803	28,803

Note: *p<0.1; **p<0.05; ***p<0.01

[19] Support for the following statement: "In order to catch criminals, do you believe that authorities can occasionally cross the line?"

[20] Support for the following statement: "Democracy is preferable to any other form of government."

[21] Binary indicator of sympathy for a party. Party 1 represents the party with most sympathizers in a given country, Party 2 the second, Party 3 the third, and Party 4 the fourth.

The findings are similar to the results obtained using panel data. Crime victimization reduces support for democracy, and increases support for strong-handed policies for reducing crime. There is no evidence that crime affects sympathy for the first, second, third or fourth-most preferred parties. This analysis allows us to increase the external validity of the results obtained in the two cities in Brazil.

3.7 Conclusions

The study of the political consequences of crime victimization is particularly necessary in countries where crime is a common phenomenon, and where candidates exploit the ideas associated with "radical penal populism" as a political strategy to gain votes. Crime victimization is a common negative event in the developing world, particularly in Latin America. This can lead to support of repression, which implies a new understanding of what the state is allowed to do to guarantee the security of its citizens. In particular, the adoption of tough policies against delinquency can foster the systematic violations of citizens' rights (Fuentes, 2005). Strong-handed measures to reduce crime tend to be present in the rhetoric of political campaigns, and many candidates emphasize their capacity to deal with crime and implement iron-fist policies to decrease victimization.

This article's findings can have important political implications. When affected citizens are more likely to support a repressive state, a rise in crime during electoral years can be exploited by populist candidates who propose iron-fist policies for controlling crime. The effect of crime victimization can have long-term consequences when it leads to the actual adoption of those policies. There is evidence of voters in the region supporting ex-authoritarian candidates accused of human right abuses because they promise to combat crime at any cost (Seligson, 2002). In this context, victims' new policy preferences can have meaningful consequences in terms of the quality of candidates elected and the policies implemented.

Previous literature has mainly focused on how voters evaluate politicians, following a classic retrospective voting approach. However, crime victimization can modify the policies voters would like to see implemented, in addition to punishing the incumbent. There

is evidence of issue voting in Latin America, which means that voters might choose the candidate whose platform best matches their policy preference (Baker and Greene, 2011; Baker and Greene, 2015). As a consequence, citizens' new policy preferences can be an important factor in understanding their electoral decisions.

Having new policy preferences, however, does not necessarily mean adopting a new party identification. Crime victimization modifies voters' policy preferences mainly by changing their democratic values, and therefore makes them more willing to support strategies that erode basic rights in the attempt to combat crime.

Studying the effects of crime is complicated, and studies that do not incorporate longitudinal data tend to have several shortcomings, such as a lack of pretreatment covariates, and an endogeneity and serial victimization problem. The statistical theory of design sensitivity shows how elements of the design can reduce sensitivity to hidden biases (Rosenbaum, 2004). I heed these recommendations to construct a more robust observational study. In particular, I focus on reducing heterogeneity, which can meaningfully decrease the impact of unmeasured confounders. Additionally, the use of panel data provides pretreatment covariates and pretreatment measures of the outcomes, which helps generate better comparisons.

Crime victimization is one of the many negative events that can modify voters' policy preferences. Economic shocks, natural disasters, and terrorist attacks may have similar implications, but they may alter voters' preferences in different directions and by different magnitudes. Unemployment and natural disasters can be associated with welfare and social policies, while terrorist attacks can be linked to the adoption of strict security measures. Therefore, we might expect voters to not just rely on a purely retrospective evaluation of the incumbent's performance when making electoral decisions, but rather also select candidates based on new policy preferences. In particular, affected citizens may prefer political authorities who want to implement the policies they most care about.

3.8 Appendices

Appendix A: Description of Covariates Panel Data

Table 3.4: Covariates included in the matching (first part)

Variable	Categories
Neighborhood (wave t)	1:44
Perceptions of safety in the neighborhood (wave t-2)	(1) Very safe, (2) Safe, (3) Little safe, (4) Not safe
Do you watch TV news? (wave t)	(1) Yes, (2) No
Do you read about politics in newspapers (wave t)	(1) Yes, (2) No
Frequency of Internet usage (wave t)	(1) Everyday, (2) Few times per week, (3) Few times per month, (4) Few times per year, (5) Never
Talk about politics with friends (wave t)	(1) Frequently, (2) Sometimes, (3) Rarely, (4) Never
Talk about politics with family (wave t)	(1) Frequently, (2) Sometimes, (3) Rarely, (4) Never
Comparison with other families from same neighborhood (wave t)	(1) Similar, (2) Different, (3) Very different
Importance of combating crime (wave t-2)	(1) Most important priority, (0) otherwise
Attention paid to presidential election (wave t-1)	(1) A lot, (2) Some, (3) A little, (4) Very little, (5) Nothing
Have you persuaded others to vote? (wave t)	(1) Yes, (2) No
Military feeling thermometer (wave t-2)	0,1,2,3,4,5,6,7,8,9,10
Union feeling thermometer (wave t-2)	0,1,2,3,4,5,6,7,8,9,10
Business sector feeling thermometer (wave t-2)	0,1,2,3,4,5,6,7,8,9,10
President (FHC) feeling thermometer (wave t)	0,1,2,3,4,5,6,7,8,9,10
Do you identify with a party? (wave t)	(1) Yes, (0) No
Importance of party when you vote (wave t)	(1) Very important, (2) Important, (3) A little important, (4) No important
Ideology (wave t)	(1) Right, (2) Center-right, (3) It depends, (4) Center-left, (5) Left
Opinions about social spending (wave t)	(1) Strongly agree, (2) Agree a little, (3) It depends, (4) Disagree a little, (5) Strongly disagree
Opinions about minimum wage (wave t)	(1) Strongly agree, (2) Agree a little, (3) It depends, (4) Disagree a little, (5) Strongly disagree
Gender (wave t)	(1) Male, (2) Female

Table 3.5: Covariates included in the matching (second part)

Variable	Categories
Education (wave t-1)	(1) No education, (2) First grade, (3) Second grade, (4) Third grade, (5) Four grade, (6) Fifth grade, (7) Sixth grade, (8) Seventh grade, (9) Eight grade, (10) First grade second level, (11) Second grade second level, (12) Third second level, (13) College incomplete, (14) College complete, (15) Graduate school incomplete, (16) Graduate school complete
Stable job	(1) Yes, (2) No
Job in the formal sector	(1) Yes, (2) No
Job in the public sector	(1) Yes, (2) No
Worried about losing job in the future	(1) A lot, (2) A little, (3) Nothing
Age (wave t)	16:90
Name of one presidential candidate (wave t)	(1) Yes, (0) No
Support for strong-handed policies to reduce crime (wave t-2)	(1) Yes, (0) No
Support for death penalty to reduce crime (wave t-2)	(1) Yes, (0) No
Support for democracy (wave t-2)	(1) Yes, (0) No
Support for death penalty (wave t-1)	(1) Yes, (0) No
Vote for Ciro (wave t)	(1) Yes, (0) No
Vote for Lula (wave t)	(1) Yes, (0) No
Vote for Serra (wave t)	(1) Yes, (0) No
Vote for Garotinho (wave t)	(1) Yes, (0) No
Do you identify with the PMDB (wave t)	(1) Yes, (0) No
Do you identify with the PFL (wave t)	(1) Yes, (0) No
Do you identify with the PSDB (wave t)	(1) Yes, (0) No
Do you identify with the PT (wave t)	(1) Yes, (0) No
White (wave t-2)	(1) Yes, (0) No
Pardo/Mestizo (wave t-2)	(1) Yes, (0) No
Black (wave t-2)	(1) Yes, (0) No
Voted for FHC in 1998 (wave t-2)	(1) Yes, (0) No
Voted for Lula in 1998 (wave t-2)	(1) Yes, (0) No
Catholic (wave t-2)	(1) Yes, (0) No
Evangelical (wave t-2)	(1) Yes, (0) No
No religion (wave t-2)	(1) Yes, (0) No

Appendix B: Summary Statistics Before Matching

Table 3.6: Descriptive statistics before matching

Statistic	N	Mean	St. Dev.
Perceptions of safety	2,236	2.68	0.75
Do you watch TV news?	2,236	1.05	0.23
Do you read about politics in newspapers	2,236	1.49	0.50
Frequency of internet usage	2,236	4.37	1.25
Talk about politics with friends	2,236	2.16	1.02
Talk about politics with family	2,236	1.80	0.90
Comparison with other families	2,236	1.32	0.55
Importance of combating crime	2,236	0.40	0.49
Attention paid to presidential election	2,236	2.38	1.27
Have you persuaded others to vote?	2,236	1.70	0.46
Military feeling thermometer	2,236	6.46	2.54
Union feeling thermometer	2,236	5.67	2.70
Business sector feeling thermometer	2,236	5.37	2.60
President (FHC) feeling thermometer	2,236	4.20	3.19
Do you identify with a party?	2,236	0.50	0.50
Importance of party when you vote	2,236	2.66	1.02
Ideology	2,236	2.88	1.38
Opinions about social spending	2,236	1.88	1.45
Opinions about minimum wage	2,236	2.16	0.93
Gender	2,236	1.57	0.49
Education	2,236	8.94	3.63
Stable job	2,236	1.59	0.49
Job in the formal sector	2,236	1.21	0.41
Job in the public sector	2,236	1.93	0.25
Worried about losing job in the future	2,236	1.89	0.31
Age	2,236	43.83	16.31
Name of one presidential candidate	2,236	0.48	0.50
Support strong-handed policies to reduce crime	2,236	0.15	0.36
Support death penalty to reduce crime	2,236	0.25	0.43
Support for democracy	2,236	0.49	0.50
Support for death penalty	2,236	0.33	0.47
Vote for Ciro	2,236	0.08	0.27
Vote for Lula	2,236	0.52	0.50
Vote for Serra	2,236	0.20	0.40
Vote for Garotinho	2,236	0.10	0.30
Do you identify with the PMDB	2,236	0.11	0.31
Do you identify with the PFL	2,236	0.01	0.10
Do you identify with the PSDB	2,236	0.02	0.14
Do you identify with the PT	2,236	0.32	0.47
White	2,236	0.54	0.50
Pardo/Mestizo	2,236	0.20	0.40
Black	2,236	0.09	0.28
Voted for FHC in 1998	2,236	0.32	0.46
Voted for Lula in 1998	2,236	0.24	0.43
Catholic	2,236	0.67	0.47
Evangelical	2,236	0.10	0.30
No religion	2,236	0.18	0.38

Appendix C: Summary Statistics After Matching

Table 3.7: Descriptive statistics after matching

Statistic	N	Mean	St. Dev.
Perceptions of safety	542	2.70	0.77
Do you watch TV news?	542	1.06	0.23
Do you read about politics in newspapers	542	1.47	0.50
Frequency of internet usage	542	4.40	1.22
Talk about politics with friends	542	2.11	1.02
Talk about politics with family	542	1.77	0.88
Comparison with other families	542	1.32	0.55
Importance of combating crime	542	0.42	0.49
Attention paid to presidential election	542	2.34	1.27
Have you persuaded others to vote?	542	1.69	0.46
Military feeling thermometer	542	6.41	2.55
Union feeling thermometer	542	5.56	2.77
Business sector feeling thermometer	542	5.34	2.51
President (FHC) feeling thermometer	542	4.21	3.28
Do you identify with a party?	542	0.52	0.50
Importance of party when you vote	542	2.69	1.03
Ideology	542	2.89	1.39
Opinions about social spending	542	1.82	1.42
Opinions about minimum wage	542	2.16	0.95
Gender	542	1.59	0.49
Education	542	9.05	3.37
Stable job	542	1.59	0.49
Job in the formal sector	542	1.22	0.42
Job in the public sector	542	1.94	0.23
Worried about losing job in the future	542	1.87	0.33
Age	542	43.34	16.09
Name of one presidential candidate	542	0.50	0.50
Support strong-handed policies to reduce crime	542	0.15	0.36
Support death penalty to reduce crime	542	0.26	0.44
Support for democracy	542	0.45	0.50
Support for death penalty	542	0.33	0.47
Vote for Ciro	542	0.07	0.25
Vote for Lula	542	0.49	0.50
Vote for Serra	542	0.23	0.42
Vote for Garotinho	542	0.11	0.31
Do you identify with the PMDB	542	0.12	0.33
Do you identify with the PFL	542	0.01	0.10
Do you identify with the PSDB	542	0.03	0.17
Do you identify with the PT	542	0.31	0.46
White	542	0.53	0.50
Pardo/Mestizo	542	0.19	0.39
Black	542	0.08	0.28
Voted for FHC in 1998	542	0.32	0.47
Voted for Lula in 1998	542	0.21	0.41
Catholic	542	0.66	0.48
Evangelical	542	0.10	0.30
No religion	542	0.19	0.39

Appendix D: Description of Panel Data

The two cities panel data was conducted between 2002 and 2006. The first wave was implemented in March/April 2002, the second in August 2002, the third in October 2002, the fourth in May 2004, the fifth in July 2006, and the sixth in October 2006.

The question that captures the main outcome of interest was only asked in wave 1 and wave 4. Wave 3 provides the baseline for the study, because I subset the sample to subjects that in that wave were not crime victims to study the impact of victimization in the following wave. Wave 1 cannot be the baseline for the study because the outcome was not asked in wave 2 and because there were no pretreatment covariates for the first wave. I refer to wave 3 as wave t. Meanwhile, waves 1 and 2 are waves $t-1$ and $t-2$. I study the effect of victimization on "strong-handed policy preferences" only in wave 4 because those questions were not included in the subsequent waves.

Because there are 19 months between wave t (October 2002) and wave $t+1$ (May 2004), it is possible that someone was a crime victim in the first 7 months after wave t and is included in the control group. That person should not have reported a crime in wave $t+1$ because this event did not happen in the previous 12 months. This should not be problematic because, in a worse case scenario, any effect can be interpreted as a conservative estimate.

I include 48 pretreatment covariates (from waves 1, 2, and 3) in the matching procedure. For missing values in the covariates I impute the median and include a binary indicator of missingness as a mean balance constraint.

I apply some data exclusion criteria. I exclude from the analysis those respondents that: (i) were crime victims in wave t, (ii) did not answer the crime victimization question in wave t, (iii) did not answer the crime victimization question in wave $t+1$.

I do not exclude units with missing outcome data because it would be too costly in terms of dropping missing values. Therefore, I construct a binary variable of support for strong-handed policies to reduce crime, support for democracy, and party identification. For example, the main outcome of interest is coded 1 when respondents support the following statement: "the best way to reduce crime is with repression and an iron fist," and

0 otherwise. Meanwhile, support for democracy is coded 1 when respondents support the statement: " democracy is always better than other forms of government," and 0 otherwise. In the sake of consistency, I follow the same approach when constructing outcomes in the external validity analysis.

For the X vector in the estimation equation, I include two predictors of the outcomes: education and age. I also add missing value indicators for these covariates.

The matching procedure was implemented by using the `Gurobi` 6.5.0 (mac64) solver and the `designmatch` packages for R.

Appendix E: More about the Matching Algorithm

After obtaining a matched sample it is possible to re-pair the units to minimize heterogeneity in the treated-minus-control response differences, which will lead to a reduction in the sensitivity to unmeasured biases (Zubizarreta, Paredes, and Rosenbaum, 2014). Following ibid., an effect τ is less sensitive to an unmeasured bias u, if the treated-minus-control response Y is tightly packed or has a compact distribution around its center. One alternative for re-pairing units is to use a Mahalanobis distance computed with covariates that are good predictors of the outcomes. I implement this post-matching step (re-pairing) using the following pretreatment covariates: support for strong-handed policies to reduce crime and a military feeling thermometer. The process of pairing for heterogeneity has no impact when using regressions, but its benefits can be observed when implementing a Rosenbaum sensitivity analysis (Rosenbaum, 2005; Zubizarreta, Paredes, and Rosenbaum, 2014).

Appendix F: Amplification of a Sensitivity Analysis

The matching procedure was able to eliminate overt biases generated by imbalances in observed covariates. However, it is still possible that certain unobserved covariates are introducing biases and then explaining the outcomes. How can we address such concerns about the possible existence of unmeasured biases?

First, design sensitivity is the effect that research design can have on sensitivity to hidden biases (Rosenbaum, 2004, 2010). For example, the statistical theory of design sensitivity recommends reducing the heterogeneity of the sample. In this chapter I attempt to achieve that goal by focusing on two cities in Brazil and generating balance at the neighborhood level,.

Second, unobserved pretreatment differences can be studied by using a sensitivity analysis, which asks how large the unmeasured covariates need to be to explain away a given effect. I implement the amplification of a Rosenbaum sensitivity analysis by using a one-sided Wilcoxon signed rank test statistic. A naive model will assume that two subjects with the same observed covariates x will have the same chance of receiving the treatment: for example, 50% each. A sensitivity analysis studies how different odds of receiving the treatment, explained by the existence of an unmeasured covariate u, can alter the conclusions of the observational study. The odds of differential assignment to the treatment are represented by the parameter Γ, and when this is equal to one it means that two units with the same observed covariates have the same chance of receiving the treatment. If this is true, the study is free of hidden biases, which can be seen as a strong assumption. The parameter Γ makes the assumption that the unobserved factor is a quite strong predictor of the outcome. Meanwhile, the amplification analysis allows us to interpret Γ in two different parameters. Λ, which controls the relationship between the hidden factor and treatment assignment. And Δ, which controls the relationship between the hidden factor and the outcome (Rosenbaum, 2015b). The amplification shows that the p-values will still be lower than 0.05 even if there is an unobserved covariate that doubles the odds of being a crime victim ($\Lambda = 2$) and increases in one and a half the odds of supporting strong-handed policies ($\Delta = 1.57$).

Appendix G: Description of Survey Data

To conduct this analysis I use the 2012 survey from the Latin American Public Opinion Project. The study conducted in 2014 (the last year available for all countries) does not ask the question about support for iron-fist policies in most of the countries. I incorporate 18 Latin American countries in the analysis: Mexico, Guatemala, El Salvador, Honduras, Nicaragua, Costa Rica, Panama, Colombia, Ecuador, Bolivia, Peru, Paraguay, Chile, Uruguay, Brazil, Venezuela, Argentina, and the Dominican Republic.

I use the following question to construct the treatment: "Have you been a victim of crime in the last 12 months?" When respondents answered "yes" the treatment was coded as a 1, and when they answered "no" it was coded as a 0.

I focus on four covariates: age, education (years of schooling), gender (1: female, 0: male), and ethnicity (mestizo: 1, white: 2, indigenous: 3, black: 4, otherwise: 5). I also include country fixed effects, and ethnicity as a factor variable (mestizo is the reference category).

Support for iron-fist policies is coded 1 when respondents support the statement, "In order to catch criminals, authorities occasionally can cross the line," and 0 otherwise. Support for democracy is coded 1 when respondents support the statement, "Democracy is preferable to any other form of government," and 0 otherwise. Finally, I generate four variables to test the impact of crime on party identification. Party 1 is coded 1 when respondents support the most preferred party in a given country, and 0 otherwise. Party 2 is coded 1 when respondents support the second-most preferred party in a given country, and 0 otherwise. Party 3 is coded 1 when respondents support the third-most preferred party in a given country, and 0 otherwise. Party 4 is coded 1 when respondents support the fourth-most preferred party in a given country, and 0 otherwise.

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